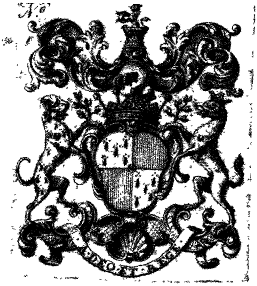
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AGRICULTURAL RESEARCH INSTITUTE
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PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXI.

PART I.

LONDON,

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MDCCCXI.

ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions,* take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions,* that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them, without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Gold and Silver Medals on COUNT RUMFORD's Donation to M. MALUS for his discoveries of certain new Properties of Reflected Light, published in the Second Volume of Mémoires d'Arcueil

PHILOSOPHICAL

TRANSACTIONS.

I. *The Bakerian Lecture. On some of the Combinations of Oxymuriatic Gas and Oxygene, and on the chemical Relations of these Principles, to inflammable Bodies. By* Humphry Davy, *Esq. LL. D. Sec. R. S. F. R. S. E. M. R. I. A. and M. R. I.*

Read November 15, 1810.

1. *Introduction.*

IN the last communication which I had the honour of presenting to the Royal Society, I stated a number of facts, which inclined me to believe, that the body improperly called in the modern nomenclature of chemistry, *oxymuriatic acid gas*, has not as yet been decompounded; but that it is a peculiar substance, elementary as far as our knowledge extends, and analogous in many of its properties to oxygene gas.

My objects in the present Lecture, are to detail a number of experiments which I have made for the purpose of illustrating more fully the nature, properties, and combinations of this substance, and its attractions for inflammable bodies, as compared with those of oxygene; and likewise to present some general views and conclusions concerning the chemical powers of different species of matter, and the proportions in which they enter into union.

I have been almost constantly employed, since the last session of the Society, upon these researches, yet this time has not been sufficient to enable me to approach to any thing complete in the investigation. But on subjects, important both in their connexion with the higher departments of chemical philosophy, and with the œconomical applications of chemistry, I trust that even these imperfect labours will not be wholly unacceptable.

2. *On the Combinations of Oxymuriatic Gas and Oxygene, with the Metals from the fixed Alkalies.*

The intensity of the attraction of potassium for oxymuriatic gas, is shewn by its spontaneous inflammation in that substance, and by the vividness of the combustion. I satisfied myself, by various minute experiments, that no water is separated in this operation, and that the proportions of the compound are such that one grain of potassium absorbs about 1.1 cubical inch of oxymuriatic gas at the mean temperature and pressure, and that they form a neutral compound, which undergoes no change by fusion. I used, in the experiments from which these conclusions are drawn, a tray of platina for receiving the potassium; the metal was heated in an exhausted vessel, to decompose any water absorbed by the crust of potash, which forms upon the potassium during its exposure to the atmosphere, and the gas was freed from vapour by muriate of lime. Large masses of potassium cannot be made to inflame, without heat in oxymuriatic gas. In all experiments in which I fused the potassium upon glass, the retorts broke in pieces in consequence of the violence of the combustion, and even in two instances when I used the tray of platina. If oxymuriatic gas be used, not freed from vapour, or if the potassium has been previously exposed to the air, a little moisture always separates during the process of combustion. When pure potassium, and pure oxymuriatic gas are used, the result, as I have stated, is a mere binary compound, the same as muriate of potash, that has undergone ignition.

The combustion of potassium and sodium in oxygene gas, is much less vivid than in oxymuriatic gas. From this phenomenon, and from some others, I was inclined to believe that the attraction of these metals for oxygene is feebler, than their attraction for oxymuriatic gas. I made several experiments, which proved that this is the fact; but before I enter upon a detail of them, it will be necessary to discuss more fully than I have yet attempted, the nature of the combinations of potassium and sodium with oxygene, and of potash and soda with water.

I have stated in the last Bakerian Lecture, that potassium and sodium, when burnt in oxygene gas, produce potash and soda in a state of extreme dryness, and very difficult of fusion. In the experiments from which these conclusions are drawn, as I mentioned, I used trays of platina, and finding that this metal was oxidated in the operation, I heated the retort strongly, to expel any oxygene the platina might have absorbed, and except in cases when this precaution was taken, I found the absorption of oxygene much greater than could be accounted for by the production of the alkalies. In all cases in which I burnt potassium or sodium in common air, applying only a gentle heat, I found that the first products were substances extremely fusible, and of a reddish brown colour, which copiously effervesced in water, and which became dry alkali, by being strongly heated upon platina in the air, phenomena, which, at an early period of the enquiry, induced me to suppose that they were prot-oxides of potassium and sodium. Finding, in subsequent experiments, however, that they deflagrated with iron filings, and rapidly oxidated platina and silver, I suspended my opinion on the subject, intending to investigate their nature more fully.

Since that time, these oxides, as I find by a notice in the Moniteur for July 5th, 1810, have occupied the attention of M. M. GAY LUSSAC and THENARD, and these able chemists have discovered that they are peroxides of potassium and sodium, the one containing, according to them, three times as much oxygene as potash, and the other 1.5 times as much as soda

I have been able to confirm in a general way these interesting results, though I have not found any means of ascertaining accurately, the quantity of oxygene contained in these new oxides. When they are formed upon metallic substances, there is always a considerable oxidation of the metal, even though platina be employed. I have used a platina tray lined with muriate of potash, that had been fused; but in this case, though I am inclined to believe that some alkali was formed at the same time with the peroxides, yet I obtained an absorpton of 2.6 cubical inches, in a case when 2 grains of potassium were employed, and of 1.63 cubical inches, in a case when a grain of sodium was used, but in this last instance, the edge of the platina tray had been acted upon by the metal, and was oxidated.* The mercury in the barometer in these experiments stood at 30.12 inches, and that in the thermometer at 62° FAHRENHEIT.

When these peroxides were formed upon muriate of potash, the colour of that from potassium was of a bright orange; that from sodium of a darker orange tint. They gave off oxygene, as M. M. GAY LUSSAC and THENARD state, by the action of water or acids. They were converted into alkali, as the French chemists have stated, by being heated with any metallic or inflammable matter. They thickened fixed oils, forming a compound that did not redden paper tinged with turmeric, without the addition of water.

When potassium is brought in contact with fused nitre, in tubes of pure glass, there is a slight scintillation only, and the nitre becomes of a red brown colour. In this operation, nitrogene is produced, and the oxide of potassium formed. I thought that by ascertaining the quantity of nitrogene evolved by the action of a given weight of potassium, and comparing this with the quantity of oxygene disengaged from the oxide by water, I might be able to determine its composition accurately. A grain of potassium acting in this way, I found produced only $\frac{16}{100}$ of nitrogene; and the red oxide by its action upon water, produced less than half a cubical inch of oxygene, so that it is probable that potash as well as its peroxide is formed in the operation.

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* M. M. GAY LUSSAC and THENARD have stated in the paper above referred to, that common potash and barytes absorb oxygene when heated. It would seem that the action of the fixed alkalies, and of barytes on platina, depends on the production of the peroxides. I have little doubt but that these ingenious gentlemen will have anticipated this observation, in the detailed account of their experiments.

Sodium, when brought in contact with fused nitre, produced a violent deflagration. In two experiments in which I used a grain of the metal, the tube broke with the violence of the explosion. I succeeded in obtaining the solid results of the deflagration of $\frac{1}{2}$ a grain of sodium, but it appeared that no peroxide had formed, for the mass gave no oxygene by the action of water.

When potassium is burnt in a retort of pure glass, the result is partly potash and partly peroxide, and by a long continued red heat the peroxide is entirely decomposed.

A grain of potassium was gently heated in a small green glass retort containing oxygene; it burnt slowly, and with a feeble flame; a quantity of oxygene was absorbed equal to $\frac{90}{100}$ of a cubical inch; by heating the retort to dull redness, oxygene was expelled equal to $\frac{38}{100}$ of a cubical inch; the mercury in the thermometer in this experiment stood at 63° FAHRENHEIT, and that in the barometer at 30.1 inches.

In experiments on the electrical decomposition of potash and soda, when the VOLTAIC battery employed contains from 500 to 1000 series in full action; the metals burn at the moment of their production, and form the peroxides; and it is probable, from the observations of M. RITTER, that these bodies may be produced likewise in VOLTAIC operations on potash, at the positive surface.

In my early experiments on potassium and sodium, I regarded the fusible substances appearing at the negative surface, in the VOLTAIC circuit, as well as those produced by the exposure of the metals to heat and air, as prot-oxides, and as similar to the results obtained by heating the metals in contact with small quantities of alkali.

I have repeated these last operations, in which I conceived that prot-oxides were formed.

Potassium and sodium, when heated in glass tubes in contact with about half of their weight of potash and soda, that have been ignited, become first of a bright azure, then produce a considerable quantity of hydrogene, and at last form a gray coherent mass, not fusible at a dull red heat, and which gives hydrogene by the action of water.

Whether these are true prot-oxides, or merely mixtures of the alkaline metals with the alkalies, or with the alkalies and reduced silex from the glass, I shall not at present attempt to decide.

Potassium I find heated in a similar manner with fused potash, in a tube of platina, gives after having been ignited, a dark mass that effervesces with water; but even in this case, it may be said that the alloy of platina and potassium interferes, and that the substance is not a protoxide, but merely dry alkali mixed with this alloy.

As the pure alkalies were unknown, till the discovery of potassium and sodium,* and as their properties have never been described, it will perhaps be proper in this place to notice them briefly.

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* STAHL approached nearly to the discovery of the pure alkalies. He cemented solid caustic potash with iron filings in a long continued heat, and states, that in this way an alkali "valde causticum" is produced. *Specim. Becb.* part ii. page 255. He procured caustic alkali also, by decomposing nitre by the metals. Id. p. 253.

I find that when nitre is decomposed in a crucible of platina, by a strong red heat, a yellow substance remains, which consists of potash and oxide of platina, apparently in chemical combination. The undecompounded potash which comes over in the process for procuring potassium by the gun-barrel, is of an olive colour, and affords oxide of iron during its solution in water. Pure potash will probably be found to have an affinity for many metallic oxides.

When potassium and sodium are burnt in oxygene gas upon platina, and heated to redness to decompose the peroxide of potassium, the alkalies are of a grayish green colour. They are harder than common potash or soda, and, as well as I could determine by an imperfect trial, of greater specific gravity. They require a strong red heat for their perfect fluidity, and evaporate slowly, by a still further increase of temperature. When small quantities of water are added to them, they heat violently, become white, and are converted into hydrats, and then are easily fusible and volatile.

When potassium or sodium is burnt on glass, freed from metallic oxides, and strongly heated, or when potash or soda is formed from the metals by the action of a minute quantity of water, their colour approaches to white; but in other sensible properties, they resemble the alkalies formed upon metallic substances; and are distinguished in a marked manner by their difficult fusibility from the potash and soda prepared by alcohol.

M. D'ARCET, and more distinctly M. BERTHOLLET, have concluded that the loss of weight of common fused potash and soda, during their combination with acids, depends upon the expulsion of water, which M. BERTHOLLET has rated at 13.9 per cent. for potash, and M. D'ARCET, at 27 or 28 for potash, and 28 or 29 for soda.*

I have stated in the last Bakerian Lecture, that my own results led me to conclude, that fused potash contained about 16 or 17 parts in the 100 of water, taking the potash formed, by adding oxygene to potassium as a standard.

The experiment from which I drew my conclusions, was made on the action of silex and potash fused together, and

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* Annales de Chimie, tom. 68, page 190.

I regarded the loss of weight, as the indication of the quantity of moisture.

I am acquainted with no experiment on record, in which water has been actually collected from the ignited fixed alkalies, and this appeared necessary for the complete elucidation of the subject.

I heated together in a green glass retort, 40 grains of potash, (that had been ignited for several minutes), and 100 grains of boracic acid, which had been heated to whiteness for nearly an hour. The retort was carefully weighed, and connected with a small receiver, which was likewise weighed; the bulb of the retort was then gradually heated till it became of a cherry red; there was a violent effervescence in the retort, a fluid condensed in the neck, and passed into the receiver. When the process was completed, the whole of the retort was strongly heated; it was found to have lost 6½ grains, and the receiver had gained 5.8 grains. The fluid that it contained was water, holding in solution a minute quantity of boracic acid, and when evaporated, it did not leave an appreciable quantity of residuum.

A similar experiment made upon soda, heated to redness, but in which the water collected was not weighed, indicated 22.9 of water in 100 parts of soda.

It may be asked, whether part of the water evolved in these processes might not have been produced from the boracic acid, or formed in consequence of its agency; but the following experiments shew that this can not be the case in any sensible degree.

I heated 8 grains of potassium, with about 50 grains of boracic acid, to redness in a tube of platina, connected with a glass tube, kept very cool; but I found that no moisture whatever was separated in the process. I mixed a few grains of potassium with red oxide of mercury, and ignited the mixture in contact with boracic acid, but no elastic product, except mercury, was evolved.

I made some potash by the combustion of potassium in a glass tube, and ignition of the peroxide, I added to it dry boracic acid, and heated the mixture to redness. Sub-borate of potash was formed, and there was not the slightest indications of the presence of moisture.*

It is evident from this chain of facts, that common potash

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* These processes must not however be considered as shewing that boracic acid that has been heated to whiteness is entirely free from water; they merely prove that such an acid gives off no water by combination with pure potash at a *red heat.* I have found that boracic acid in perfect fusion, and that has been long exposed to the blast of a forge, and that has long ceased to effervesce, gives globules of hydrogene; when dry iron filings are made to act upon it. I added to 54 grains of boracic acid in complete fusion, in a crucible of platina, 75 grains of flint glass that had been previously heated to whiteness, and immediately reduced into powder in a hot iron mortar; by raising the heat so as to produce combination, a copious effervescence was produced; and after intense ignition for half an hour, the mixture was found to have lost three grains and a quarter.

The combinations of boracic acid with potash and soda, that have been heated to redness, I find lose weight when their temperature is raised to a much higher degree. Thus, in an experiment made in the laboratory of my friend JOHN GEORGE CHILDREN, ESQ. and in which Mr. CHILDREN was so kind as to co-operate, 71 grains of hydrat of potash, mixed with 96 of boracic acid that had been heated as strongly as possible in a blast furnace, lost by fusion together in a red heat 11 grains, but on raising the temperature to whiteness the loss increased to above 13 grains. 55,5 grains of hydrat of soda, mixed with 80 of boracic acid, examined at intervals in a process of this kind, continued to lose weight for half an hour, during which time they were frequently heated to whiteness; at the end of this period the whole loss was 14 grains, of which at least one grain and a half may be referred to the acid. 95 grains of soda, ignited to whiteness in a platina crucible, with 140 of dry flint glass, lost 22.2 grains; 80 grains of boracic glass were added to this mixture; a fresh effer-

and soda are hydrats, and the bodies formed by the combustion of the alkaline metals, are, as I have always stated, pure metallic oxides, (as far as our knowledge extends) free from water.[*]

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vescence took place, and after intense ignition for a few minutes, there was an additional loss of weight of four grains and a half. The energy with which water adheres to certain bodies in other cases, is shewn by the experiments of M. Berthollet, *Mem. d'Arcueil*, tom. ii. page 47. Indeed it is impossible to say that a neutral compound, or a fixed acid is ever entirely free from water; it is only the first proportions that are easily separated. If the proportions of water in common potash and soda were to be judged of from their loss of weight, in combining with boracic acid, it would appear to be from 19 to 20 per cent. in the first, and from 23 to 25 in the second.

[*] After the experiments detailed in my two last papers, it may perhaps appear unnecessary, at least to those enlightened British chemical philosophers who have closely followed the progress of science, to offer any new evidences to prove that potassium and sodium are not hydrurets of potash and soda, particularly as M. M. Gay Lussac and Thenard, the ingenious advocates of this notion have acknowledged, in the Moniteur to which I have before referred, that it is not tenable; but on a subject so intimately connected with the most refined departments of chemical philosophy, and with so many new objects of research, additional facts cannot be wholly devoid of use and application.

Mr. Dalton, in the second volume of the work which he entitles "*A New System of Chemical Philosophy*," of which he has had the goodness to send me a copy, has, I find in his first pages, adopted the idea that potash and soda are metallic oxides; but in the latter pages has considered them as simple bodies, and the metals formed from them as compounds of potash and soda with hydrogene. He has given no facts in favour of this change in his opinion: his principal argument is founded upon the process in which I first obtained potassium. Common potash is a hydrat: when oxygene is procured from this by Voltaic electricity at one surface, and potassium at the other surface; Mr. Dalton conceiving that this oxygene arises from the water, states that the hydrogene of the water must combine with the potash to form potassium. It is evident, that adopting such a plan of reasoning, lead or copper might be proved to be hydrurets of their oxides; for when these metals are revived from their aqueous acid solutions, oxygene is produced at the positive surface, and no hydrogene at the negative surface.

In my first experiments for producing potassium and sodium, I used a weak

I shall now resume the detail of the experiments that I have made, on the relative attractions of oxymuriatic gas and oxygene, for the metals of the fixed alkalies. I burnt a grain of potassium in oxygene gas, in a retort of green glass, furnished with a stop-cock, and heated the oxide formed, to redness, to convert it into potash: half a cubical inch of oxygene was absorbed. The retort was exhausted, and very pure oxymuriatic gas admitted. The

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power, and in these instances procuring the metals in very small quantities only, I perceived no effervescence. When from five hundred to one thousand plates are used for producing potassium, there is a violent effervescence, and a production of hydrogene and sometimes of potassuretted hydrogene, connected with the formation of the metal.

Potassium, brought in contact with red hot hydrat of potash, disengages abundance of hydrogene, and the whole is converted into difficultly fusible potash.

327 grains of hydrat of potash that had been ignited, were made to act in a gun-barrel on 745 grains of iron turnings heated to whiteness. Some hydrogene was lost, and some hydrat of potash remained undecompounded, yet 225 cubical inches of inflammable gas were collected, and 50 grains of potassium, and a large quantity of an alloy of potassium and iron formed, so that it is scarcely possible to doubt that all the hydrogene produced from the decomposed hydrat of potash was liberated.

Mr. DALTON conceives that there is an analogy between potassium and sodium, and the compounds of hydrogene with sulphur, phosphorus, and arsenic; but I am at loss to trace any similarity between sulphureted hydrogene, which is a gaseous body, soluble in water, and having acid properties, and a highly inflammable solid metal which produces alkali by combustion. Potassium might as well be compared to carbonic acid. Mr. DALTON considers the volatility of potassium and sodium as favouring the idea of their containing hydrogene; but they are less volatile than antimony, arsenic, and tellurium, and much less volatile than mercury. He mentions their low specific gravity as a circumstance favourable to this idea. I have on a former occasion examined this argument, first brought forward by M. RITTER; but it may not be amiss to add, that if potassium is a compound of hydrogene and potash, hydrat of potash must contain an equal quantity of hydrogene, with the addition of a light gaseous element, oxygene, which might be expected to diminish rather than to increase the specific gravity of the compound. Mr. DALTON states, p. 488, that potassium forms dry hydrat of potash, by decomposing nitrous gas colour of the potash instantly became white, and by a gentle heat, the whole was converted into muriate of potash: a cubical inch and $\frac{1}{8}$ of oxymuriatic gas were absorbed, and exactly half a cubical inch of oxygene generated. The barometer during this operation was at 30.3, the thermometer at 62 FAHRENHEIT. I made several experiments of the same kind, but this is the only one on which I can place entire dependence. When I attempted to use larger quantities of potassium, the retort usually broke during the cooling of the glass, and it was not possible to gain any accurate results in employing metallic trays. The potassium was spread into a thin plate, and of course was much oxidated before its[*] admission into the retort, which rendered the absorption of oxygene a little less than it ought to have been. In the process it was heated in vacuo before the combustion, to decompose the water in the crust of potash; for in cases when this precaution was not taken, I found that hydrat of potash sublimed, and lined the upper part of the retort, and from this the oxymuriatic gas separated water as well as oxygene.

The phenomenon of the separation of water from hydrat of potash by oxymuriatic gas, was happily exemplified in an experiment in which I introduced oxymuriatic gas to the peroxide of potassium, formed in a large retort, and in which

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and nitrous oxide; this is not the case: and he does not refer to experiment. I find by some very careful trials, that potassium attracts the oxygene and some of the nitrogene from these bodies, and forms a fusible compound which may be decomposed, giving off nitrogene and its excess of oxygene, by a red heat, and which becomes *potash*, and not dry hydrat of potash.

M. M. GAY LUSSAC and THENARD have convinced themselves that potassium and sodium are not hydrurets of potash and soda, by a method similar to that which I adopted and published some months before, namely, by producing neutral salts from them.

the potassium had been covered with a considerable crust of hydrat of potash. The upper part of the retort and its neck contained a white sublimate of hydrat, which had risen in combustion, and which was perfectly opaque. As soon as the gas was admitted, it instantly became transparent from the evolution of water; and on heating the glass in contact with the sublimate, its opacity was restored, and water driven off.

In various cases in which I heated dry potash, or mixtures of potash and the peroxide, in oxymuriatic gas, there was no separation of moisture, except when the gas contained aqueous vapour; and the oxygene evolved in the process, when the heat was strongly raised, exactly corresponded to that absorbed by the potassium.

When muriatic acid gas was introduced to potash formed from the combustion of potassium, water was instantly formed, and oxymuriate of potassium.* I have made no accurate experiment on the proportions of muriatic acid gas decomposed by potash, but I made a very minute investigation, of the nature of the mutual decomposition of this substance, and hydrat of potash.

Ten grains of hydrat of potash were heated to redness in a tray of platina, which was carefully weighed; it was introduced into a retort which was exhausted of air, and the retort was filled with muriatic acid gas. The hydrat of potash was heated by a spirit lamp; water instantly separated in great abundance, and muriate of potash formed. A strong heat was applied till the process was completed, when the tray was taken out and weighed; it had gained $2\frac{13}{16}$ grains. A minute quantity of liquid muriatic acid was added to the muriate, to ensure a complete neutralization, and the tray heated to redness: there was no additional increase of weight.

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* i. e. Muriate of potash.

In the few experiments which I have made on the action of sodium and soda on oxymuriatic gas, the phenomena appeared precisely analogous; but sodium, as might have been expected, absorbed nearly twice as much oxymuriatic gas as potassium.

When common salt that has been ignited, is heated with potassium, there is an immediate decomposition, and by giving the mixture a red heat, pure sodium is obtained; and this process affords an easy mode, and the one I have always lately adopted for procuring that metal. No hydrogene is disengaged in this operation, and two parts of potassium I find produces rather more than one of sodium.

From the series of proportions that I have communicated in my last paper, it is evident that 1 grain of potassium ought to absorb 1.08 cubical inches of oxymuriatic acid; and that the potash formed from one grain of potassium ought to decompose about 2.16 cubical inches of muriatic acid gas; and these estimations agree very nearly with the result of experiments.

The estimation of the composition of soda, as deduced from the experiments in the last Bakerian lecture, is 25.4 of oxygene to 74.6 of metal, and this would give the number representing the proportion in which sodium combines with bodies 22.;* from which it is evident, that a grain of sodium ought to absorb nearly 2 cubical inches of oxymuriatic gas, and that the same quantity converted into soda, would decompose nearly four cubical inches of muriatic gas. Muriate of soda ought on this idea to contain one proportion of sodium, 22., and one of oxymuriatic gas 32.9; and this estimation is very near that which may be gained from Dr. MARCET's analysis of this substance. Hydrat of potash ought to consist of 1 proportion of potash, represented by 48., and one of water, represented by 8.5. This gives its composition as 15.1 of water, and 84.9 of potash. Hydrat of soda ought, according to theory, to contain 1 proportion of soda 29.5, and 1 of water 8.5, which will give in 100 parts 22.4 of water; and the experiments that I have detailed, conform as well as can be expected with these conclusions.

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* Or if soda be considered as deutoxide, which seems probable from the experiments detailed page 4, 44; and on this supposion, the salts of soda must be conceived to contain double proportions of acid. On either datum the proportion of oxygene in water must be taken as 7.5, and that of hydrogene as 1, though other numbers might be found as divisors or multiples of those which would equally harmonise with the general doctrine of definite proportions. In my last communication to the Society, I have quoted Mr. DALTON as the original Author of the hypothesis,

The proportions of potash and soda indicated, in different neutral combinations, by these estimations, will be found to agree very nearly with those derived from the most accurate analysis, particularly those of M. BERTHOLLET; or the differences are such as admit of an easy explanation.

I stated in my last communication, the probability that the oxygene in the hyperoxymuriate of potash was in triple combination with the metal and oxymuriatic gas; the new facts

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that water consists of 1 particle of oxygene, and 1 of hydrogene; but I have since found that this opinion is advanced, in a work published in 1789. *A comparative View of the Phlogistic and Antiphlogistic Theories,* by WILLIAM HIGGINS. In this elaborate and ingenious performance, Mr. HIGGINS has developed many happy sketches of the manner in which (on the corpuscular hypothesis) the particles or molecules of bodies may be conceived to combine; and some of his views, though formed at this early period of investigation, appear to me to be more defensible, assuming his data, than any which have been since advanced; for instance, he considered nitrous gas as composed of two particles of oxygene, and one of nitrogene. Mr. HIGGINS had likewise drawn the just conclusion respecting the constitution of respecting the peroxide confirm this idea. Potassium, perfectly saturated with oxygene, would probably contain six

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sulphuretted hydrogene, from its electrical decomposition. As hydrogene is the substance which combines with other bodies in the smallest quantity, it is perhaps the most fitted to be represented by unity; and on this idea the proportions in ammonia will be 3 of hydrogene to 1 of nitrogene, and the number representing the smallest proportion in which nitrogene is known to combine will be 13.4. Mr. Dalton, *New System of Chemical Philosophy*, pages 323 and 436, has adopted 4.7 or 5.1, as the number representing the weight of the *atom* of nitrogene; and has quoted my experiments, *Researches, Chemical and Philosophical*, as authorising these numbers; but all the enquiries on nitric acid, nitrous gas, nitrous oxide, and on the decomposition of nitrat of ammonia stated in that work, conform much more nearly to the number 13.4.

According to Mr. Dalton, nitrat of ammonia contains one proportion of acid and one of alkali, and nitrate of potash two proportions of acid and one of alkali; but it is easy to see that the reverse must be the case. Nitrate of ammonia is known to be an acid salt; and nitrate of potash a neutral salt; which harmonizes with the views above stated. Mr. Dalton estimates the quantity of water in nitric acid of specific gravity 1.54, at 27.5 per cent.; and this, according to him, is a stronger acid than he obtained by decomposing fused nitre by sulphuric acid, which contained only 19 per cent. of water, and one quantity of sulphuric acid, according to him, will produce from nitre, more than an equal weight of nitric acid, and he supposes no water in nitre; so that his conclusion as to the quantity of water in liquid nitric acid on his own data must be incorrect. I find water in fused nitre, by decomposing it by boracic acid.

I shall enter no further at present into an examination of the opinions, results, and conclusions of my learned friend; I am however obliged to dissent from most of them, and to protest against the interpretations that he has been pleased to make of my experiments; and I trust to his judgment and candour for a correction of his views.

It is impossible not to admire the ingenuity and talent with which Mr. Dalton has arranged, combined, weighed, measured, and figured his atoms; but it is not, I conceive, on any speculations upon the ultimate particles of matter, that the true theory of definite proportions must ultimately rest. It has a surer basis in the mutual decomposition of the neutral salts, observed by Richter and Guyton de Morveau, in the mutual decompositions of the compounds of hydrogene and nitrogene, of nitrogene and oxygene, of water and the oxymuriatic compounds; in the multiples of oxygene in the nitrous compounds; and those of acids in salts, observed by Drs. Wollaston proportions; for, according to Mr. CHENEVIX's analysis, which is confirmed by one made in the Laboratory of the Royal Institution, by Mr. E. DAVY, hyperoxymuriate of potash must consist of 40.5 potassium, 32.9 oxymuriatic gas, and 45 of oxygene.

I have mentioned, that by strongly heating the peroxide of potassium in oxymuriatic acid, all the oxygene is expelled, and a mere combination of oxymuriatic gas and potassium formed. I thought it possible, that at a low temperature, a combination might be effected, and I have reason to believe that this is the case. I made a peroxide of potassium, by heating potassium with about twice the quantity of nitre, and admitted oxymuriatic gas which was absorbed: some oxygene was expelled on the fusion of the peroxide, but a salt remained, which gave oxymuriatic gas, as well as muriatic acid, by the action of sulphuric acid.

It seems evident, that in the formation of the hyperoxymuriate of potash, one quantity of potash is decomposed by the attraction of oxymuriatic gas to form muriate of potash; but the oxygene, instead of being set free in the nascent state, enters into combination with another portion of potash, to form a peroxide, and with oxymuriatic gas.

The proportions required for these changes may be easily deduced from the data which have been stated in the preceding pages. 5 proportions of potash, equal to 240 grains, must be decomposed to form with an equal number of proportions

and THOMSON; and above all, in the decompositions by the VOLTAIC apparatus. Where oxygene and hydrogene, oxygene and inflammable bodies, acids and alkalies, &c. must separate in uniform ratios.

of oxymuriatic gas equal to 164.5 grains, 5 proportions of muriate of potash equal to 367 grains; and 5 of oxygene equal to 37, 5 grains, combined with one of potash, equal to 48, must unite in triple union with one of oxymuriatic gas equal to 32.9, to form one proportion, equal to 118.4 grains of hyperoxymuriate of potash.

3. *On the Combinations of the Metals of the Earths, with Oxygene and Oxymuriatic Gas.*

The muriates of baryta, lime, and strontia, after being a long time in a white heat, are not decomposable by any simple attractions: thus, they are not altered by dry boracic acid, though, when water is added to them, they readily afford muriatic acid and their peculiar earths.

From this circumstance, I was induced to believe that these three compounds consist merely of the peculiar metallic bases, which I have named barium, strontium, and calcium, and oxymuriatic gas; and such experiments as I have been able to make, confirm the conclusion.

When baryta, strontia, or lime, is heated in oxymuriatic gas to redness, a body precisely the same as a dry muriate is formed, and oxygene is expelled from the earth. I have never been able to effect so complete a decomposition of these earths by oxymuriatic gas, as to ascertain the quantity of oxygene produced from a given quantity of earth. But in three experiments made with great care I found that one of oxygene was evolved for every two in volume of oxymuriatic gas absorbed.

I have not yet tried the experiment of acting upon oxymuriatic gas by the bases of the alkaline earths; but I have not the least doubt that these bodies would combine directly with that substance, and form dry muriates.

In the last experiments that I made on the metallization of the earths by amalgamation, I paid particular attention to the state of the products formed, by exposing the residuum of amalgams to the air. I found that baryta formed in this way was not fusible at an intense white heat, and that strontia and lime so formed gave off no water when ignited. Baryta made from chrystals of the earth, as M. BERTHOLLET has shewn, is a fusible hydrat, and I found that this earth gave moisture when decomposed by oxymuriatic gas; and the lime, in hydrat of lime, was much more rapidly decomposed by oxymuriatic gas than quicklime, its oxygene being rapidly expelled with the water.

Some dry quicklime was heated in a retort, filled with muriatic acid gas; water was instantly formed in great abundance, and it can hardly be doubted, that this arose from the hydrogene of the acid combining with the oxygene of the lime.

As potassium so readily decomposes common salt, I thought it might possibly decompose muriate of lime, and thus afford easy means of procuring calcium. The rapidity with which muriate of lime absorbs water, and the difficulty of freeing it even by a white heat from the last portions, rendered the circumstances of the experiments unfavourable. I found, however, that by heating potassium strongly, in contact with the salt, in a retort of difficultly fusible glass, I obtained a dark coloured matter, diffused through a vitreous mass, which effervesced strongly with water. The potassium had all disappeared, and the retort had received a heat at which potassium entirely volatilizes. I had similar results with muriate of strontia, and (though less distinct, more potassium distilling off unaltered) with muriate of baryta. Either the bases of the earths were wholly or partially deprived of oxymuriatic gas in these processes, or the potassium had entered into triple combination with the muriates. I hope on a future occasion to be able to decide this point.

Combinations of muriatic acid gas with magnesia, alumine and silex, are all decomposed by heat, the acid being driven off, and the earth remaining free. I conjectured from this circumstance, that oxymuriatic gas would not expel oxygene from these earths, and the suspicion was confirmed by experiments. I heated magnesia, alumine, and silex to redness in oxymuriatic gas, but no change took place.

M. M. GAY LUSSAC and THENARD have shewn that baryta is capable of absorbing oxygene; and it seems likely, (as according to Mr. CHENEVIX's experiments, most of the earths are capable of becoming hyperoxymuriates) that peroxides of their bases must exist.

I endeavoured to combine lime with more oxygene, by heating it in hyperoxymuriate of potash, but without success, at least after this process it gave off no oxygene in combining with water. The salt, called oxymuriate of lime, made for the use of the bleachers, I found gave off oxygene by heat, and formed muriate of lime.

From the proportions which I have given in the last Bakerian lecture, but which were calculated from the analyses of sulphates, it follows that if the muriates of baryta, strontia, and lime, be regarded as containing one proportion of oxymuriatic gas, and one of metal, then they would consist of 71* barium, 46 strontium, and 21 calcium, to 32.9 of oxymuriatic gas.

To determine how far these numbers are accurate, 50 grains of each of these muriates that had been heated to whiteness were decomposed by nitrate of silver, the precipitate was collected, washed, heated, and weighed.

The muriate of baryta, treated in this way, afforded 68 grains of horn-silver.

The muriate of strontia 85 grains.

The muriate of lime 125 grains.

From experiments to be detailed in the next section, it appears that horn-silver consists of 12 of silver to 3.9 of oxymuriatic gas, and consequently that barium should be represented by 65.1, strontium by 46.1, and calcium by 20.8.

4. *On the Combinations of the Common Metals, with Oxygene and Oxymuriatic Gas.*

In the limits which it is usual to adopt in this lecture, it will not be possible for me to give more than an outline of the numerous experiments that I have made on the combinations of oxymuriatic gas with metals; I must confine myself to a general statement of the mode of operating, and the results. I used in all cases small retorts of green glass, containing from 3 to 6 cubical inches, furnished with stopcocks. The metallic substances were introduced, the retort exhausted and filled with the gas to be acted upon, heat was applied by means of a spirit lamp, and after cooling, the results were examined, and the residual gas analysed.

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* If Mr. JAMES THOMPSON's analysis of sulphate of barytes be made the basis of calculation, sulphuric acid being estimated as 36, then the number representing barium will be about 65.5,

All the metals that I tried, except silver, lead, nickel, cobalt, and gold, when heated, burnt in the oxymuriatic gas, and the volatile metals with flame. Arsenic, antimony, tellurium and zinc with a white flame, mercury with a red flame. Tin became ignited to whiteness, and iron and copper to redness; tungsten and manganese to dull redness; platina was scarcely acted upon at the heat of fusion of the glass.

The product from arsenic was butter of arsenic; a dense, limpid, highly volatile fluid, a non-conductor of electricity, and of high specific gravity, and which when decomposed by water, gave oxide of arsenic and muriatic acid. That from antimony, was butter of antimony, an easily fusible and volatile solid, of the colour of horn-silver, of great density, crystallizing on cooling in hexahedral plates, and giving, by its decomposition by water, white oxide.

The product from tellurium, in its sensible qualities, resembled that from antimony, and gave when acted on by water white oxide.

The product from mercury was corrosive sublimate. That from zinc was similar in colour to that from antimony, but was much less volatile.

The combination of oxymuriatic gas and iron, was of a bright brown; but having a lustre approaching to the metallic, and was iridescent like the Elba iron ore. It volatilized at a moderate heat, filling the vessel with beautiful minute crystals of extraordinary splendour, and collecting in brilliant plates, the form of which I could not determine. When acted on by water, it gave red muriate of iron.

Copper formed a bright red brown substance, fusible at a heat below redness, and becoming crystalline and semi-transparent on cooling, and which gave a green fluid, and a green precipitate by the action of water.*

The substance from manganese was not volatile at a dull red heat; it was of a deep brown colour, and by the action of water became of a brighter brown: a muriate of manganese, which did not redden litmus remained in solution; and an insoluble matter remained of a chocolate colour.†

Tungsten afforded a deep orange sublimate, which, when decomposed by water, afforded muriatic acid, and the yellow oxide of tungster.

Tin afforded LIBAVIUS's liquor, which gave a muriate by the action of water containing the oxide of tin, at the maximum of oxidation.

Silver and lead produced horn-silver and horn-lead, and bismuth, butter of bismuth. The absorption of oxymuriatic gas was in the following proportions for two grains of each of the metals: for arsenic 3.6 cubical inches, for antimony

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* It is worth enquiry, whether the precipitate from oxymuriate of copper by water is not a hydrated submuriate, analogous in its composition to the crystalized muriate of Peru. This last I find affords muriatic acid and water by heat.

The *resin of copper* discovered by BOYLE, formed by heating copper with corrosive sublimate, probably contains only 1 proportion of oxymuriatic gas, whilst that above referred to must contain 2.

† When muriate of manganese is made by solution of its oxide in muriatic acid, a neutral combination is obtained, but this is decomposed by heat; muriatic gas flies off, and brown oxide of manganese remains. In this respect manganese appears as a link between the ancient metals and the newly discovered ones. Its muriate is decomposed like that of magnesia; and its oxide is the only one amongst those long known, as far as my experiments have gone, which neutralizes the acid energy of muriatic acid gas, so as to prevent it in solution from affecting vegetable blues.

3.1, for tellurium 2.4, for mercury 1.05,* for zinc 3 2, for iron 5.8, for tin 4, for bismuth 1.5, for copper 3.4, for lead .9, for silver, the absorption of volume was $\frac{9}{10}$, and the increase of weight of the silver was equivalent to $\frac{6}{10}$ of a grain.†

In acting upon metallic oxides by oxymuriatic gas, I found that those of lead, silver, tin, copper, antimony, bismuth, and tellurium, were decomposed in a heat below redness, but the oxides of the volatile metals, more readily than those of the fixed ones. The oxides of cobalt and nickel were scarcely acted upon at a dull red heat. The red oxide of iron was not affected at a strong red heat, whilst the black oxide was rapidly decomposed at a much lower temperature; arsenical acid underwent no change at the greatest heat that could be given it in the glass retort, whilst the white oxide readily decomposed.

In cases where oxygene was given off, it was found exactly the same in quantity as that which had been absorbed by the metal. Thus 2 grains of red oxide of mercury absorbed $\frac{9}{10}$ of a cubical inch of oxymuriatic gas, and afforded 0.45 of oxygene.‡ Two grains of dark olive oxide from calomel decomposed by potash, absorbed about $\frac{94}{100}$ of oxymuriatic gas, and afforded $\frac{24}{100}$ of oxygene, and corrosive sublimate was produced in both cases.

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* The gas in these experiments was not freed from aqueous vapour, and as stopcocks of brass were used, a little gas might have been absorbed by the surface of this metal, so that the processes offer only approximations to the composition of the oxymuriates. The processes on lead, tellurium, iron, antimony, copper, tin, mercury, and arsenic, were carried on in three successive days, during which the height of the mercury in the barometer varied from 30.26 inches to 30.15, and the height of that in the thermometer from 63 5 to 61 FAHRENHEIT.

The experiment on silver was made at the temperature of 52 FAHRENHEIT, and under a pressure equal to that of 29 9 inches.

† This agrees nearly with another experiment made by my brother, Mr. JOHN DAVY, in which 12 grains of silver increased to 15.9 during their conversion into horn-silver.

‡ I have made two analyses of corrosive sublimate and calomel, with considerable

In the decomposition of the white oxide of zinc, oxygene was expelled exactly equal to half the volume of the oxymuriatic acid absorbed. In the case of the decomposition of the black oxide of iron, and the white oxide of arsenic, the changes that occurred were of a very beautiful kind; no oxygene was given off in either case, but butter of arsenic, and arsenical acid formed in one instance, and the ferruginous sublimate, and red oxide of iron in the other.

care. I decomposed 100 grains of corrosive sublimate, by 90 grains of hydrat of potash. This afforded 79.5 grains of orange coloured oxide of mercury, 40 grains of which afforded 9.15 cubical inches of oxygene gas; the muriate of silver formed from the 100 grains was 102.5.

100 grains of calomel, decomposed by 90 grains of potash, afforded 82 grains of olive coloured oxide of mercury, of which 40 grains gave by decomposition by heat 4.8 cubical inches of oxygene. The quantity of horn-silver formed from the 100 grains was 58.75 grains.

In the second analysis, the quantity of oxide obtained from corrosive sublimate was 78.7; the quantity of muriate of silver formed was 103 4; the oxide produced from calomel weighed 83 grains; the horn-silver formed was 57½ grains. I am inclined to put most confidence in the last analyses; but the tenor of both is to shew that the quantity of oxymuriatic gas in corrosive sublimate, is exactly double that in calomel, and that the orange oxide contains twice as much oxygene as the black, the mercury being considered as the same in all. The olive colour of the oxide formed from calomel, is owing to a slight admixture of orange oxide, formed by the oxygene of the water used in precipitation; the tint I find is almost black, when a boiling solution of potash is used; and trituration, with a little orange oxide brings the tint to olive. It has been stated, that the olive oxide thrown down from calomel by potash is a submuriate; but I have never been able to find a vestige of muriatic acid in it when well washed. It is not easy to obtain perfect precision in analyses of the oxides of mercury; water adheres to the oxides, which cannot be entirely driven off without the expulsion of some oxygene. In all my experiments, though the oxides had been heated to a temperature above 212, a little dew collected in the neck of the retort, so that the 40 grains must have been over-rated.

Two grains of white oxide of arsenic absorbed 0.8 of oxymuriatic gas.*

I doubt not that the same phenomena will be found to occur in other instances, in which the metal has comparatively a slight attraction only for oxymuriatic gas, and when it is susceptible of different degrees of oxydation, and in which the peroxide is used.

The only instance in which I tried to decompose a common metallic oxide, by muriatic acid, was in that of the fawn coloured oxide of tin; water rapidly separated, and LIBAVIUS's liquor was formed.

From the proportions which may be gained in considering the volumes of oxymuriatic gas absorbed by the different metals, in their relations to the quantity of oxygene which would be required to convert them into oxides, it would appear, that in the experiments to which I have referred, either one, two, or three proportions of oxymuriatic gas combine with one of metal, and consequently, from the composition of the muriates, it will be easy to obtain the numbers representing the proportions in which these metals may be conceived to enter into other compounds.†

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* A singular instance of the tendency of the oxide of arsenic to become arsenical acid, occurs in its action on fused hydrat of potash, the water in the hydrat is rapidly decomposed, and arseniuretted hydrogene evolved, and arseniate of potash formed.

† From the experiments detailed in the note in the opposite page, it would appear that the number representing the proportion in which mercury combines must be about 200. That of silver, as would appear from the results, page 25, about 100. The numbers of other metals may be learnt from the data in the same page, but from what has been stated, these data cannot be considered as very correct.

5. *General Conclusions and Observations, illustrated by Experiments.*

All the conclusions which I ventured to draw in my last communication to the Society, will, I trust, be found to be confirmed by the whole series of these new enquiries.

Oxymuriatic gas combines with inflammable bodies, to form simple binary compounds; and in these cases, when it acts upon oxides, it either produces the expulsion of their oxygene, or causes it to enter into new combinations.

If it be said that the oxygene arises from the decomposition of the oxymuriatic gas, and not from the oxides, it may be asked, why it is always the quantity contained in the oxide; and why in some cases, as those of the peroxides of potassium and sodium, it bears no relation to the quantity of gas?

If there existed any acid matter in oxymuriatic gas, combined with oxygene, it ought to be exhibited in the fluid compound of one proportion of phosphorus, and two of oxymuriatic gas; for this, on such an assumption, should consist of muriatic acid (on the old hypothesis, free from water) and phosphorous acid; but this substance has no effect on litmus paper, and does not act under common circumstances, on fixed alkaline bases, such as dry lime or magnesia. Oxymuriatic gas, like oxygene, must be combined in large quantity with peculiar inflammable matter, to form acid matter. In its union with hydrogene, it instantly reddens the driest litmus paper, though a gaseous body. Contrary to acids, it expels oxygene from protoxides, and combines with peroxides.

When potassium is burnt in oxymuriatic gas, a dry compound is obtained. If potassium combined with oxygene is employed, the whole of the oxgene is expelled, and the same compound formed. It is contrary to sound logic to say, that this exact quantity of oxygene is given off from a body not known to be compound, when we are certain of its existence in another; and all the cases are parallel.

An argument in favour of the existence of oxygene in oxymuriatic gas, may be derived by some persons from the circumstances of its formation, by the action of muriatic acid on peroxides, or on hyperoxymuriate of potash; but a minute investigation of the subject will, I doubt not, shew that the phænomena of this action are entirely consistent with the views I have brought forward. By heating muriatic acid gas in contact with dry peroxide of manganese, water I found was rapidly formed, and oxymuriatic gas produced, and the peroxide rendered brown. Now as muriatic acid gas is known to consist of oxymuriatic gas and hydrogene, there is no simple explanation of the result, except by saying that the hydrogene of the muriatic acid, combined with oxygene from the peroxide to produce water.

SCHEELE explained the bleaching powers of the oxymuriatic gas, by supposing that it destroyed colours by combining with phlogiston. BERTHOLLET considered it as acting by supplying oxygene. I have made an experiment, which seems to prove that the pure gas is incapable of altering vegetable colours, and that its operation in bleaching depends entirely upon its property of decomposing water, and liberating its oxygene.

I filled a glass globe containing dry powdered muriate of lime, with oxymuriatic gas. I introduced some dry paper tinged with litmus that had been just heated, into another globe containing dry muriate of lime; after some time this globe was exhausted, and then connected with the globe containing the oxymuriatic gas, and by an appropriate set of stopcocks, the paper was exposed to the action of the gas. No change of colour took place, and after two days there was scarcely a perceptible alteration.

Some similar paper dried, introduced into gas that had not been exposed to muriate of lime, was instantly rendered white.*

Paper that had not been previously dried, brought in contact with dried gas, underwent the same change, but more slowly.

The hyperoxymuriates seem to owe their bleaching powers entirely to their loosely combined oxygene; there is a strong tendency in the metal of those in common use, to form simple combinations with oxymuriatic gas, and the oxygene is easily expelled or attracted from them.

It is generally stated in chemical books, that oxymuriatic gas is capable of being condensed and crystallized at a low temperature; I have found by several experiments that this is not the case. The solution of oxymuriatic gas in water freezes more readily than pure water, but the pure gas dried by muriate of lime undergoes no change whatever, at a temperature of 40 below 0° of FAHRENHEIT. The mistake seems to have arisen from the exposure of the gas to cold in botttles containing moisture.

I attempted to decompose boracic and phosphoric acids by oxymuriatic gas, but without success; from which it seems probable, that the attractions of boracium and phosphorus for oxygene are stronger than for oxymuriatic gas. And from the experiments I have already detailed, iron and arsenic are analogous in this respect, and probably some other metals.

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* The last experiments were made in the laboratory of the Dublin Society; most of the preceding ones in the laboratory of the Royal Institution; and I have been permitted to refer to them by the Managers of that useful public establishment.

Potassium, sodium, calcium, strontium, barium, zinc, mercury, tin, lead, and probably silver, antimony, and gold seem to have a stronger attraction for oxymuriatic gas than for oxygene.

I have as yet been able to make very few experiments on the combinations of the oxymuriatic compounds with each other, or with oxides. The liquor from arsenic, and that from tin, mix, producing an increase of temperature; and the phosphuretted, and the sulphuretted liquors unite with each other, and with the liquor of LIBAVIUS, but without any remarkable phenomena.

I heated lime gently in a green glass tube, and passed the phosphoric sublimate, the saturated oxymuriate of phosphorus through it, in vapour; there was a violent action with the production of heat and light, and a gray fused mass was formed, which afforded by the action of water, muriate and phosphate of lime.

I introduced some vapour from the heated phosphoric sublimate, into an exhausted retort containing dry paper tinged with litmus: the colour slowly changed to pale red. This fact seems in favour of the idea that the substance is an acid; but as some minute quantity of aqueous vapour might have been present in the receiver, the experiment cannot be regarded as decisive: the strength of its attraction for ammonia, is perhaps likewise in favour of this opinion. All the oxymuriates that I have tried, indeed form triple compounds with this alkali; but phosphorus is expelled by a gentle heat from the other compounds of oxymuriatic gas and phosphorus with ammonia, and the substance remaining in combination is the phosphoric sublimate.

6. *Some Reflections on the Nomenclature of the Oxymuriatic Compounds.*

To call a body which is not known to contain oxygene, and which cannot contain muriatic acid, oxymuriatic acid, is contrary to the principles of that nomenclature in which it is adopted; and an alteration of it seems necessary to assist the progress of discussion, and to diffuse just ideas on the subject. If the great discoverer of this substance had signified it by any simple name, it would have been proper to have recurred to it; but, dephlogisticated marine acid is a term which can hardly be adopted in the present advanced æra of the science.

After consulting some of the most eminent chemical philosophers in this country, it has been judged most proper to suggest a name founded upon one of its obvious and characteristic properties—its colour, and to call it *Chlorine*, or *Chloric* gas.*

Should it hereafter be discovered to be compound, and even to contain oxygene, this name can imply no error, and cannot necessarily require a change.

Most of the salts which have been called muriates, are not known to contain any muriatic acid, or any oxygene. Thus LIBAVIUS's liquor, though converted into a muriate by water, contains only tin and oxymuriatic gas, and horn-silver seems incapable of being converted into a true muriate.

I venture to propose for the compounds of oxymuriatic gas and inflammable matter, the name of their bases, with the termination *ane.* Thus argentane may signify horn-silver; stannane, LIBAVIUS's liquor; antimonane, butter of antimony; sulphurane, Dr. THOMSON's sulphuretted liquor; and so on for the rest.

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* From χλωρος.

In cases when the proportion is one quantity of oxymuriatic gas, and one of inflammable matter, this nomenclature will be competent to express the class to which the body belongs, and its constitution. In cases when two or more proportions of inflammable matter, combine with one of gas; or two or more of gas, with one of inflammable matter, it may be convenient to signify the proportions by affixing vowels before the name, when the inflammable matter predominates, and after the name, when the gas is in excess; and in the order of the alphabet, *a* signifying two, *e* three, *i* four, and so on.

The name muriatic acid, as applied to the compound of hydrogene and oxymuriatic gas, there seems to be no reason for altering. And the compounds of this body with oxides should be characterized in the usual manner, and as the other neutral salts.

Thus muriate of ammonia and muriate of magnesia, are perfectly correct expressions.

I shall not dwell any longer at present upon this subject.—What I have advanced, I advance merely as suggestion, and principally, for the purpose of calling the attention of philosophers to it.* As chemistry improves, many other alterations will be necessary; and it is to be hoped that whenever they take

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* It may be conceived that a name may be found for the oxymuriatic gas in some modification of its present appellation which may harmonize with the new views, and which may yet signify its relation to the muriatic acid, such as demuriatic gas, or oxymuric gas; but in this case it would be necessary to call the muriatic acid, hydrogenated muriatic acid, or hydromuriatic acid; and the salts which contain it

hydrogenated muriates or hydromuriates; and on such a plan, the compounds of oxymuriatic gas must be called demuriates or oxymuriates, which I conceive would create more complexity and difficulty in unfolding just ideas on this department of chemical knowledge than the methods which I have ventured to propose. It may however be right, considering the infant state of the investigation, to suspend, for a a time, the adoption of any new terms for these compounds. It is possible that oxymuriatic gas may be compound, and that this body and oxygene may contain some common principle; but at present we have no more right to say that oxymuriatic gas contains oxygene than to say that tin contains hydrogene; and names should express things and not opinions; and till a body is decompounded, it should be considered as simple.

In the last number of Mr. NICHOLSON's Journal, which appeared February 1st, whilst this sheet was correcting for the press, I have seen an ingenious paper, by Mr. MURRAY, of Edinburgh, in which he has attempted to shew, that oxymuriatic gas contains oxygene. His methods are, by detonating oxymuriatic gas in excess, with a mixture of hydrogene, and gaseous oxide of carbone, when he *supposes* carbonic acid is formed; and by mixing oxymuriatic gas in excess, with sulphuretted hydrogene, when he *supposes* sulphuric acid, or sulphureous acid is formed. In some experiments, in which my brother, Mr. JOHN DAVY, was so good as to co-operate, made over boiled mercury, we found, that 7 parts of hydrogene, 8 parts of gaseous oxide of carbone, and 20 parts of oxymuriatic gas, exploded by the electric spark, diminished to about 30 measures; and calomel was formed on the sides of the tube. On adding dry ammonia in excess, and exposing the remainder to water, a gas remained, which equalled more than 9 measures, and which was gaseous oxide of carbone, with no more impurity than might be expected from the air in the gasses, and the nitrogene expelled from the ammonia; so that the oxygene in Mr. MURRAY's carbonic acid, it seems, was obtained from *water*, or from the carbonic oxide. Sulphuretted hydrogene, added over dry mercury, to oxymuriatic gas in excess, inflamed in two or three experiments; muriatic acid gas containing the vapour of oxymuriate of sulphur, was formed, which, when neutralized by ammonia, gave muriate of ammonia, and a combination of ammonia, and oxymuriate of sulphur.

When a mixture of oxymuriatic gas in excess, and sulphuretted hydrogene, was suffered to pass into the atmosphere, the smell was that of oxymuriate of sulphur; there was not the slightest indication of the presence of any sulphuric or sulphureous acid. If Mr. MURRAY had used ammonia, instead of water, for analyzing his results, I do not think he would have concluded, that oxymuriatic gas is capable of decomposition by such methods.

I shall not, at present, enter upon a detail of other experiments which I have made place, they will be made independent of all speculative views, and that new names will be derived from some simple and invariable property, and that mere arbitrary designations will be employed, to signify the class to which compounds or simple bodies belong.

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on this subject, in co-operation with my brother, as it is his intention to refer to them, in an answer to Mr. MURRAY's paper.

I shall conclude, by saying, that this ingenious chemist, has mistaken my views, in supposing them hypothetical; I merely state what I have seen, and what I have found, There *may* be oxygene in oxymuriatic gas; but I can find none. I repeated Mr. MURRAY's experiments with great interest; and their results, when *water* is excluded, entirely confirm all my ideas on the subject, and afford no support to the hypothetical ideas, which he has laboured so zealously to defend.

II. *The Croonian Lecture, on some Physiological Researches, respecting the Influence of the Brain on the Action of the Heart, and on the Generation of animal Heat. By Mr.* B. C. Brodie, *F.R.S.*

Read December 20, 1810.

Having had the honour of being appointed, by the President of the Royal Society, to give the Croonian Lecture, I trust that the following facts and observations will be considered as tending sufficiently to promote the objects, for which the Lecture was instituted. They appear to throw some light on the mode, in which the influence of the brain is necessary to the continuance of the action of the heart; and on the effect, which the changes produced on the blood in respiration have on the heat of the animal body.

In making experiments on animals to ascertain how far the influence of the brain is necessary to the action of the heart, I found that when an animal was pithed by dividing the spinal marrow in the upper part of the neck, respiration was immediately destroyed, but the heart still continued to contract circulating dark-coloured blood, and that in some instances from ten to fifteen minutes elapsed before its action had entirely ceased. I further found that when the head was removed, the divided blood vessels being secured by a ligature, the circulation still continued, apparently unaffected by the entire separation of the brain. These experiments confirmed the observations of Mr. CRUIKSHANK * and M. BICHAT,† that the brain is not directly necessary to the action of the heart, and that when the functions of the brain are destroyed, the circulation ceases only in consequence of the suspension of respiration. This led me to conclude, that, if respiration was produced artificially, the heart would continue to contract for a still longer period of time after the removal of the brain. The truth of this conclusion was ascertained by the following experiment.

Experiment 1.

I divided the spinal marrow of a rabbit in the space between the occiput and atlas, and having made an opening into the trachea, fitted into it a tube of elastic gum, to which was connected a small pair of bellows, so constructed that the lungs might be inflated, and then allowed to empty themselves. By repeating this process once in five seconds, the lungs being each time fully inflated with fresh atmospheric air, an artificial respiration was kept up. I then secured the blood-vessels in the neck, and removed the head, by cutting through the soft parts above the ligature, and separating the occiput from the atlas. The heart continued to contract, apparently with as much strength and frequency as in a living animal. I examined the blood in the different sets of vessels, and found it dark-coloured in the venæ cavæ and pulmonary artery, and of the usual florid red colour in the pulmonary veins and aorta. At the end of twenty-five minutes from the time of the spinal marrow being divided, the action of the heart became fainter, and the experiment was put an end to.

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* Philosophical Transactions 1795.

† Récherches Physiologiques sur la Vie et la Mort.

With a view to promote the enquiry instituted by the Society for promoting the knowledge of animal chemistry respecting the influence of the nerves on the secretions,* I endeavoured to ascertain whether they continued after the influence of the brain was removed. In the commencement of the experiment I emptied the bladder of its contents by pressure; at the end of the experiment the bladder continued empty.

This experiment led me to conclude, that the action of the heart might be made to continue after the brain was removed, by means of artificial respiration, but that under these circumstances the secretion of urine did not take place. It appeared, however, desirable to repeat the experiment on a larger and less delicate animal, and that, in doing so, it would be right to ascertain whether, under these circumstances, the animal heat was kept up to the natural standard.

Experiment 2.

I repeated the experiment on a middle-sized dog. The temperature of the room was 63° of Fahrenheit's thermometer. By having previously secured the carotid and vertebral arteries, I was enabled to remove the head with little or no hæmorrhage. The artificial respirations were made about twenty-four times in a minute. The heart acted with regularity and strength.

At the end of 30 minutes from the time of the spinal marrow being divided, the heart was felt through the ribs contracting 76 times in a minute.

At 35 minutes the pulse had risen to 84 in a minute.

At one hour and 30 minutes the pulse had risen to 88 in a minute.

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* Philosophical Transactions for 1809.

At the end of two hours it had fallen to 70, and at the end of two hours and a half to 35 in a minute, and the artificial respiration was no longer continued.

By means of a small thermometer with an exposed bulb, I measured the animal heat at different periods.

At the end of an hour the thermometer in the rectum had fallen from 100° to 94°.

At the end of two hours a small opening being made in the parietes of the thorax, and the ball of the thermometer placed in contact with the heart, the mercury fell to 86°, and half an hour afterwards in the same situation it fell to 78°.

In the beginning of the experiment I made an opening into the abdomen, and having passed a ligature round each artery about two inches below the kidney, brought the edges of the wound in the abdomen together by means of sutures. At the end of the experiment no urine was collected in the ureters above the ligatures.

On examining the blood in the different vessels, it was found of a florid red colour in the arteries, and of a dark colour in the veins, as under ordinary circumstances.

During the first hour and a half of the experiment there were constant and powerful contractions of the muscles of the trunk and extremities, so that the body of the animal was moved in a very remarkable manner, on the table, on which it lay, and twice there was a copious evacuation of fæces.

Experiment 3.

The experiment was repeated on a rabbit. The temperature of the room was 60°. The respirations were made from 30 to 35 in a minute. The actions of the heart at first were strong and frequent: but at the end of one hour and forty minutes the pulse had fallen to 24 in a minute.

The blood in the arteries was seen of a florid red, and that in the veins of a dark colour.

A small opening was made in the abdominal muscles, through which the thermometer was introduced into the abdomen, and allowed to remain among the viscera.

At the end of an hour the heat in the abdomen had fallen from 100° to 89°. At the end of an hour and forty minutes in the same situation the heat had fallen to 85°, and when the bulb of the thermometer was placed in the thorax in contact with the lungs the mercury fell to 82°.

It has been a very generally received opinion that the heat of warm-blooded animals is dependant on the chemical changes produced on the blood by the air in respiration. In the two last experiments the animals cooled very rapidly, notwithstanding the blood appeared to undergo the usual changes in the lungs, and I was therefore induced to doubt whether the above mentioned opinion respecting the source of animal heat is correct. No positive conclusions however could be deduced from these experiments. If animal heat depends on the changes produced on the blood by the air in respiration, its being kept up to the natural standard, or otherwise, must depend on the quantity of air inspired, and on the quantity of blood passing through the lungs in a given space of time: in other words, it must be in proportion to the fullness and frequency of the pulse, and the fullness and frequency of the inspirations. It therefore became necessary to pay particular attention to these circumstances.

Experiment 4.

The experiment was repeated on a dog of a small size, whose pulse was from 130 to 140 in a minute, and whose respirations, as far as I could judge, were performed from 30 to 35 times in a minute.

The temperature of the room was 63°. The heat in the rectum of the animal at the commencement of the experiment was 99°. The artificial inspirations were made to correspond as nearly as possible to the natural inspirations both in fullness and frequency.

At 20 minutes from the time of the dog being pithed, the heart acted 140 times in a minute with as much strength and regularity as before: the heat in the rectum had fallen to 96½.

At 40 minutes the pulse was still 140 in a minute: the heat in the rectum 92½.

At 55 minutes the pulse was 112, and the heat in the rectum 90°.

At one hour and 10 minutes the pulse beat 90 in a minute, and the heat in the rectum was 88°.

At one hour and 25 minutes the pulse had sunk to 30, and the heat in the rectum was 85°. The bulb of the thermometer being placed in the bag of the pericardium, the mercury stood at 85°, but among the viscera of the abdomen it rose to 87½.

During the experiment there were frequent and violent contractions of the voluntary muscles, and an hour after the experiment was begun, there was an evacuation of fæces.

Experiment 5.

The experiment was repeated on a rabbit, whose respirations, as far as I could judge, were from 30 to 40 in a minute, and whose pulse varied from 130 to 140 in a minute. The temperature of the room was 57°. The heat in the rectum, at the commencement of the experiment, was 101½. The artificial respirations were made to resemble the natural respirations as much as possible, both in fullness and frequency.

At 15 minutes from the time of the spinal marrow being divided, the heat in the rectum had fallen to 98½°.

At the end of half an hour the heart was felt through the ribs, acting strongly 140 times in a minute.

At 45 minutes the pulse was still 140; the heat in the rectum was 94°.

At the end of an hour the pulse continued 140 in a minute; the heat in the rectum was 92°; among the viscera of the abdomen 94°; in the thorax, between the lungs and pericardium, 92°.

During the experiment, the blood in the femoral artery was seen to be of a bright florid colour, and that in the femoral vein of a dark colour, as usual.

The rabbit voided urine at the commencement of the experiment; at the end of the experiment, no urine was found in the bladder.

Experiment 6.

I procured two rabbits of the same colour, but one of them was about one-fifth smaller than the other. I divided the spinal marrow of the larger rabbit between the occiput and atlas. Having secured the vessels in the neck, and removed the head, I kept up the circulation by means of artificial respiration as in the former experiments. The respirations were made as nearly as possibble similar to natural respirations.

In 23 minutes after the spinal marrow was divided, the pulse was strong, and 130 in a minute: the ball of the thermometer being placed among the viscera of the abdomen, the mercury stood at 96°.

At 34 minutes the pulse was 120 in a minute; the heat in the abdomen was 95°.

At the end of an hour the pulse could not be felt, but on opening the thorax the heart was found acting, but slowly and feebly. The heat in the abdomen was 91°; and between the lobes of the right lung 88°.

During the experiment, the blood in the arteries and veins was seen to have its usual colour.

In this therefore, as in the preceding experiments, the heat of the animal sunk rapidly, notwithstanding the continuance of the respiration. In order to ascertain whether any heat at all was generated by this process, I made the following comparative experiment. The temperature of the room being the same, I killed the smaller rabbit by dividing the spinal marrow between the occiput and atlas. In consequence of the difference of size, *cæteris paribus*, the heat in this rabbit ought to diminish more rapidly than in the other; and I therefore examined its temperature at the end of 52 minutes, considering that this would be at least equivalent to examining that of the larger rabbit at the end of an hour. At 52 minutes from the time of the smaller rabbit being killed, the heat among the viscera of the abdomen was 92°, and between the lobes of the right lung it was 91°. From this experiment, therefore, it appeared not only that no heat was generated in the rabbit, in which the circulation was maintained by artificial respiration, but that it even cooled more rapidly than the dead rabbit.

At the suggestion of Professor DAVY, who took an interest in the enquiry, I repeated the foregoing experiment on two animals, taking pains to procure them more nearly of the same size and colour.

Experiment 7.

I procured two large full grown rabbits, of the same colour, and so nearly equal in size, that no difference could be detected by the eye.

The temperature of the room was 57°, and the heat in the rectum of each rabbit previous to the experiment was 100¼.

I divided the spinal marrow in one of them, produced artificial respiration, and removed the head after having secured the vessels in the neck. The artificial respirations were made about 35 times in a minute.

During the first hour, the heart contracted 144 times in a minute.

At the end of an hour and a quarter the pulse had fallen to 136 in a minute, and it continued the same at the end of an hour and a half. At the end of an hour and forty minutes the pulse had fallen to 90 in a minute, and the artificial respiration was not continued after this period.

Half an hour after the spinal marrow was divided, the heat in the rectum had fallen to 97°.

At 45 minutes the heat was 95½.

At the end of an hour the heat in the rectum was 94°.

At an hour and a quarter it was 92°.

At an hour and a half it was 91°.

At an hour and forty minutes, the heat in the rectum was 90½, and in the thorax, within the bag of the pericardium, the heat was 87½.

The temperature of the room being the same, the second rabbit was killed by dividing the spinal marrow, and the temperature was examined at corresponding periods.

Half an hour after the rabbit was killed, the heat in the rectum was 99°.

At 45 minutes it had fallen to 98°.

At the end of an hour the heat in the rectum was 96½.

At an hour and a quarter it was 95°.

At an hour and a half it was 94°.

At an hour and forty minutes the heat in the rectum was 93°, and in the bag of the pericardium 90½.

The following table will shew the comparative temperature of the two animals at corresponding periods.

Time.	Rabbit with artificial respiration.		Dead Rabbit.	
	Therm. in the Rectum.	Therm. in the Pericardium.	Thermometer in the Rectum.	Therm. in the Pericardium.
Before the Experiment	100½		100½	
30 min.	97		99	
45 —	95½		98	
60 —	94		96½	
75 —	92		95	
90 —	91		94	
100 —	90½	87½	93	90½

In this experiment, the thorax, even in the dead animal, cooled more rapidly than the abdomen. This is to be explained by the difference in the bulk of these two parts. The rabbit in which the circulation was maintained by artificial respiration, cooled more rapidly than the dead rabbit, but the difference was more perceptible in the thorax than in the rectum. This is what might be expected, if the production of animal heat does not depend on respiration, since the cold air by which the lungs were inflated, must necessarily have abstracted a certain quantity of heat, particularly as its influence was communicated to all parts of the body, in consequence of the continuance of respiration.

It was suggested that some animal heat might have been generated, though so small in quantity as not to counterbalance the cooling powers of the air thrown into the lungs. It is difficult or impossible, to ascertain with perfect accuracy, what effect cold air thrown into the lungs would have on the temperature of an animal under the circumstances of the last experiment, independently of any chemical action on the blood: since, if no chemical changes were produced, the circulation could not be maintained, and if the circulation ceased, the cooling properties of the air must be more confined to the thorax, and not communicated in an equal degree to the more distant parts. The following experiment, however, was instituted, as likely to afford a nearer approximation to the truth, than any other that could be devised.

Experiment 8.

I procured two rabbits of the same size and colour: the temperature of the room was 64°. I killed one of them by dividing the spinal marrow, and immediately, having made an opening into the left side of the thorax, I tied a ligature round the base of the heart, so as to stop the circulation. The wound in the skin was closed by a suture. An opening was then made into the trachea, and the apparatus for artificial respiration being fitted into it, the lungs were inflated, and then allowed to collapse as in the former experiment, about 36 times in a minute. This was continued for an hour and a half, and the temperature was examined at different periods. The temperature of the room being the same, I killed the second rabbit in the same manner, and measured the temperature at corresponding periods. The comparative temperature of the two dead animals, under these circumstances, will be seen in the following table.

Time.	Dead Rabbit whose lungs were inflated.		Dead Rabbit whose lungs were not inflated.	
	Therm. in the Rectum.	Therm. in the Thorax.	Therm. in the Rectum.	Therm. in the Thorax
Before the experiment.	100		100	
30 min.	97		98	
45 —	95½		96	
60 —	94		94½	
75 —	92½		93	
90 —	91	86	9½	88½

In this last experiment, as may be seen from the above table, the difference in the temperature of the two rabbits, at the end of an hour and a half, in the rectum, was half a degree, and in the thorax two degrees and a half; whereas, in the preceding experiment, at the end of an hour and forty minutes, the difference in the rectum was 2½ degrees, and in the thorax 3 degrees. It appears, therefore, that the rabbit in which the circulation was maintained by artificial respiration, cooled more rapidly on the whole than the rabbit whose lungs were inflated in the same manner after the circulation had ceased. This is what might be expected, if no heat was produced by the chemical action of the air on the blood: since in the last case the cold air was always applied to the same surface, but in the former it was applied always to fresh portions of blood, by which its cooling powers were communicated to the more distant parts of the body.

In the course of the experiments which I have related, I was much indebted to several members of the Society for promoting the Knowledge of Animal Chemistry, for many important suggestions which have assisted me in prosecuting the enquiry.

Mr. HOME, at my request, was present at the seventh experiment. Dr. E. N. BANCROFT was present at, and assisted me in the second experiment; and Mr. WILLIAM BRANDE lent me his assistance in the greater part of those which were made. I have been further assisted in making the experiments by Mr. BROUGHTON, surgeon of the Dorsetshire Regiment of Militia, and Mr. RICHARD RAWLINS, and Mr. ROBERT GATCOMBE, students in Surgery.

I have selected the above from a great number of similar experiments, which it would be needless to detail. It is sufficient to state, that the general results were always the same; and that whether the pulse was frequent or slow, full or small, or whether the respirations were frequent or otherwise, there was no perceptible difference in the cooling of the animal.

From the whole we may deduce the following conclusions:

1. The influence of the brain is not directly necessary to the action of the heart.

2. When the brain is injured or removed, the action of the heart ceases, only because respiration is under its influence, and if under these circumstances respiration is artificially produced, the circulation will still continue.

3. When the influence of the brain is cut off, the secretion of urine appears to cease, and no heat is generated; notwithstanding the functions of respiration, and the circulation of the blood continue to be performed, and the usual changes in the appearance of the blood are produced in the lungs.

4. When the air respired is colder than the natural temperature of the animal, the effect of respiration is not to generate, but to diminish animal heat

On the Expansion of any Functions of Multinomials. By Thomas Knight, *Esq. Communicated by* Humphry Davy, *Esq. LL.D. Sec. R. S.*

Read June 7th, 1810.

1. THE expansion of multinomial functions has, of late, been so ably and fully treated by M. ARBOGAST, in his learned work '*Du Calcul des Dérivations*,' that it may appear, perhaps, scarcely necessary to add any thing to what has been written, on the subject, by that excellent geometer.

Nevertheless, as he is the only one that has hitherto cultivated this part of analysis with any *great* success; and as it is agreeable, I believe, to most persons, to be presented with various solutions to mathematical problems, I hope it will not be thought superfluous if I show how the same things may be accomplished in a very different manner.

By the procedure here made use of, we shall also be enabled to arrive at many new and remarkable theorems (both for *direct* and *inverse derivation*), which could not, I imagine, be very easily found by M. ARBOGAST's methods.

For a function of *one* simple multinomial, I give (amongst others) the same rules of *direct derivation*, as that author; but when there are *many*, and in the more difficult cases of double and triple multinomials, &c. or functions of any number of these, I offer new and expeditious methods; which are demonstrated with the less trouble, from the analogy which reigns throughout, in this manner of treating the subject; and the regularity with which we proceed from the easy to the more complex cases. By means of this analogy also, the reader may without difficulty keep all the rules in his memory.

2. I shall begin with *the expansion of any function of a simple multinomial.*

*First method.** If $f(c+z)$ represent any function of $c+z$, and the fluxions be taken, separately, with respect to c and z, the fluxional coefficient is the same in both cases: or

$\left(\frac{\dot{\overline{f(c+z)}}}{\dot{c}}\right)=\left(\frac{\dot{\overline{f(c+z)}}}{\dot{z}}\right)$; whence it follows, that $\int\left(\frac{\dot{\overline{f(c+z)}}}{\dot{c}}\right)\dot{z}=\int\left(\frac{\dot{\overline{f(c+z)}}}{z}\right)\dot{z}=f(c+z)$. This being premised, let

$$f(c+\overset{\prime}{c}x+\overset{\prime\prime}{c}x^2+\overset{\prime\prime\prime}{c}x^3+)=B+B_1x+B_2x^2+B_3x^3+\ldots\ldots+B_nx^n+, \text{\&c.} \ldots\ldots (1);$$

let $\overset{\prime}{B}$, $\overset{\prime}{B}_1$, $\overset{\prime}{B}_2$, &c. represent the fluxional coefficients of B, B_1, B_2, &c. with respect to c, and we shall have

$$\left(\frac{\dot{\overline{f(c+cx+cx^2+cx^3+)}}}{\dot{c}}\right)=\overset{\prime}{B}+\overset{\prime}{B}_1x+\overset{\prime}{B}_2x^2+\overset{\prime}{B}_3x^3+\ldots\ldots+\overset{\prime}{B}_nx^n+.$$

If we multiply this by $\overset{\prime}{c}\dot{x}+2\overset{\prime\prime}{c}x\dot{x}+3\overset{\prime\prime\prime}{c}x^2\dot{x}+\ldots\ldots+n\overset{\prime\prime..n}{c}x^{n-1}\dot{x}+$ and take the fluent, we shall get, by what was just now shewn, another expansion of $f(c+\overset{\prime}{c}x+\overset{\prime\prime}{c}x^2+\overset{\prime\prime\prime}{c}x^3+)$;

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* See LA CRIOX, '*Traité élément. de Calc. Différent.*' p. 25, note; where a similar proceeding is used for binomial functions.

and by comparing the coefficients, of the different powers of x, with those in equation (1), there will be found,

$$\underset{1}{B} = \overset{\prime}{c}\,\overset{\prime}{B}$$

$$\underset{2}{B} = \frac{2\overset{\prime\prime}{c}\,\overset{\prime}{B} + \overset{\prime}{c}\,\underset{1}{\overset{\prime}{B}}}{2}$$

$$\underset{3}{B} = \frac{3\overset{\prime\prime\prime}{c}\,\overset{\prime}{B} + 2\overset{\prime\prime}{c}\,\underset{1}{\overset{\prime}{B}} + \overset{\prime}{c}\,\underset{2}{\overset{\prime}{B}}}{3}$$

. . . .

$$\underset{n}{B} = \frac{n\overset{\prime\prime\ldots n}{c}\,\overset{\prime}{B} + (n-1)\overset{\prime\prime..(n-1)}{c}\,\underset{1}{\overset{\prime}{B}} + (n-2)\overset{\prime\prime..(n-2)}{c}\,\underset{2}{\overset{\prime}{B}} + \ldots + 2\overset{\prime\prime}{c}\,\underset{n-2}{\overset{\prime}{B}} + \overset{\prime}{c}\,\underset{n-1}{\overset{\prime}{B}}}{n}$$

..... (2).

But $B = f(c)$, $\overset{\prime}{B} = \overset{\prime}{f}(c)$, whence all the rest are known. I represent by strokes over the f the fluxional coefficients of $f(c)$; the number of strokes marking the order.

Though this is a complete solution of the problem, it affords by no means an easy way of calculating the coefficients; on which account I shall not trouble the reader with examples. It will be shewn presently, that the method of derivation in M. ARBOGAST's first section is easily obtained from this.

3. *Second Method.* I here, as in the former case, consider the quantity $c + \overset{\prime}{c}\,x + \overset{\prime\prime}{c}\,x^2 + \overset{\prime\prime\prime}{c}\,x^3 +$ to be a binomial, and take the fluxional coefficient of the function with respect to c; but multiply by the partial fluxion $\overset{\prime}{c}\cdot x$, instead of $\overset{\prime}{c}\,\dot{x} + 2\overset{\prime\prime}{c}\,x\,\dot{x}$ +, &c.; we find, by this way of proceeding, $\int \underset{n-1}{\overset{\prime}{B}}\,\overset{\prime}{c}\cdot$ for the sum of all those terms in $\underset{n}{B}$ that are multiplied by the powers

of $\overset{\prime}{c}$. In like manner, $\int \underset{n-2}{\overset{\prime}{B}} \overset{\prime\prime}{c}\cdot$ will be the sum of all those terms, in the same coefficient, that contain $\overset{\prime\prime}{c}$ and its powers; and, in general, $\int \underset{n-m}{\overset{\prime}{B}} \overset{\prime\prime.\,.m}{c}\cdot$ the sum of those that have for factors the powers of $\overset{\prime\prime\ldots m}{c}$.

Hence is derived an easy method of finding any coefficient, when we know those that precede it: for if these partial values be united, there arises

$\underset{n}{B} = \int \underset{n-1}{\overset{\prime}{B}} \overset{\prime}{c}\cdot + \int \underset{n-2}{\overset{\prime}{B}} \overset{\prime\prime}{c}\cdot + \int \underset{n-3}{\overset{\prime}{B}} \overset{\prime\prime\prime}{c}\cdot +$, &c. .... (3), provided that we neglect

in $\underset{n-2}{B}$ all those terms which contain $\overset{\prime}{c}$,

in $\underset{n-3}{B}$ all those which contain - $\overset{\prime}{c}$ or $\overset{\prime\prime}{c}$,

in $\underset{n-4}{B}$ all those which have - $\overset{\prime}{c}$ or $\overset{\prime\prime}{c}$ or $\overset{\prime\prime\prime}{c}$,

and so on; whence it happens, that many of the B's will be neglected entirely, and the chief part of the operation will always be in the first term $\int \underset{n-1}{\overset{\prime}{B}} \overset{\prime}{c}\cdot$. From equation (3), we find the first part of the expansion of $f(c + \overset{\prime}{c}x + \overset{\prime\prime}{c}x^2 +)$ to be

$$\begin{array}{l|l|l|l|l}
f(\overset{*}{c})+\overset{\prime}{f}(c)\overset{\prime}{c}x+\overset{\prime}{f}(c)\overset{\prime\prime}{c} & x^2+\overset{\prime}{f}(c)\overset{*}{\overset{\prime\prime\prime}{c}} & x^3+\overset{\prime}{f}(c)\overset{*}{\overset{\prime\prime\prime\prime}{c}} & x^4+\overset{\prime}{f}(c)\overset{\prime\prime\prime\prime\prime}{c} & x^5+ \\
\qquad\qquad +\overset{\prime\prime}{f}(c)\frac{\overset{\prime}{c}^2}{2} & +\overset{\prime\prime}{f}(c)\overset{\prime}{c}\overset{\prime\prime}{c} & +\overset{\prime\prime}{f}(c)\left\{\overset{\prime}{c}\overset{\prime\prime\prime}{c}+\frac{\overset{*}{\overset{\prime\prime}{c}}^2}{2}\right\} & +\overset{\prime\prime}{f}(c)\left\{\overset{\prime}{c}\overset{\prime\prime\prime\prime}{c}+\overset{\prime\prime}{c}\overset{\prime\prime\prime}{c}\right\} & \\
 & +\overset{\prime\prime\prime}{f}(c)\frac{\overset{\prime}{c}^3}{2.3} & +\overset{\prime\prime\prime}{f}(c)\frac{\overset{\prime}{c}^2}{2}\overset{\prime\prime}{c} & +\overset{\prime\prime\prime}{f}(c)\left\{\frac{\overset{\prime}{c}^2}{2}\overset{\prime\prime\prime}{c}+\overset{\prime}{c}\frac{\overset{\prime\prime}{c}^2}{2}\right\} & \\
 & & +\overset{\prime\prime\prime\prime}{f}(c)\frac{\overset{\prime}{c}^4}{2.3.4} & +\overset{\prime\prime\prime\prime}{f}(c)\frac{\overset{\prime}{c}^3}{2.3}\overset{\prime\prime}{c} & \\
 & & & +\overset{\prime\prime\prime\prime\prime}{f}(c)\frac{\overset{\prime}{c}^5}{2.3.4.5} &
\end{array}$$

But, that we may enter rather more into particulars, let it be required from the terms already given, to find $\underset{6}{B}$ the coefficient of x^6.

To make the operation plain, I have put a star over every term we are to use, excepting the coefficient of x^5, which is wholly employed.

$$\int \underset{6-1}{\overset{\prime}{B}}\,\overset{\prime}{c}\cdot = \int \underset{5}{\overset{\prime}{B}}\,\overset{\prime}{c}\cdot = \overset{\prime\prime}{f}(c)\,\overset{\prime}{c}\,\overset{\prime\prime\prime\prime}{c} + \overset{\prime\prime\prime}{f}(c)\left|\begin{array}{l}\frac{\overset{\prime}{c}^2}{2}\overset{\prime\prime\prime\prime}{c}\\ +\overset{\prime}{c}\,\overset{\prime\prime}{c}\,\overset{\prime\prime\prime}{c}\end{array}\right. + \overset{\prime\prime\prime\prime}{f}(c)\left|\begin{array}{l}\frac{\overset{\prime}{c}^3}{2.3}\overset{\prime\prime\prime}{c}\\ +\frac{\overset{\prime}{c}^2\,\overset{\prime\prime}{c}^2}{2.2}\end{array}\right. + \overset{\prime\prime\prime\prime\prime}{f}(c)\,\frac{\overset{\prime}{c}^4}{2.3.4}\overset{\prime\prime}{c} + \overset{\prime\prime\prime\prime\prime\prime}{f}(c)\,\frac{\overset{\prime}{c}^6}{2.3.4.5.6};$$

$$\int \underset{4}{\overset{\prime}{B}}\,\overset{\prime\prime}{c}\cdot = \overset{\prime\prime}{f}(c)\,\overset{\prime\prime}{c}\,\overset{\prime\prime\prime}{c} + \overset{\prime\prime\prime}{f}(c)\,\frac{\overset{\prime\prime}{c}^3}{2.3};\ \int \underset{3}{\overset{\prime}{B}}\,\overset{\prime\prime\prime}{c}\cdot = \overset{\prime\prime}{f}(c)\,\frac{\overset{\prime\prime\prime}{c}^2}{2};\ \int \overset{\prime}{B}\,\overset{\prime\prime..6}{c}\cdot = \overset{\prime}{f}(c)\,\overset{\prime\prime..6}{c};$$

and by adding these together, we get

$$\underset{6}{B} = \overset{\prime}{f}(c)\,\overset{\prime\prime..6}{c} + \overset{\prime\prime}{f}(c)\left|\begin{array}{l}\overset{\prime}{c}\,\overset{\prime\prime\prime\prime\prime}{c}\\ +\overset{\prime\prime}{c}\,\overset{\prime\prime\prime\prime}{c}\\ +\frac{\overset{\prime\prime\prime}{c}^2}{2}\end{array}\right. + \overset{\prime\prime\prime}{f}(c)\left|\begin{array}{l}\frac{\overset{\prime}{c}^2}{2}\overset{\prime\prime\prime\prime}{c}\\ +\overset{\prime}{c}\,\overset{\prime\prime}{c}\,\overset{\prime\prime\prime}{c}\\ +\frac{\overset{\prime\prime}{c}^3}{2.3}\end{array}\right. + \overset{\prime\prime\prime\prime}{f}(c)\left|\begin{array}{l}\frac{\overset{\prime}{c}^3}{2.3}\overset{\prime\prime\prime}{c}\\ +\frac{\overset{\prime}{c}^2\,\overset{\prime\prime}{c}^2}{2.2}\end{array}\right. + \overset{\prime\prime\prime\prime\prime}{f}(c)\,\frac{\overset{\prime}{c}^4}{2.3.4}\overset{\prime\prime}{c} + \overset{\prime\prime\prime\prime\prime\prime}{f}(c)\,\frac{\overset{\prime}{c}^6}{2.3.4.5.6}.$$

This process is sufficiently easy; but, in order to find any coefficient as $\underset{n}{B}$, it is by no means necessary for us to know all those that precede it; it may be immediately obtained from $\underset{n-1}{B}$ by a variety of ways: but we must first learn how to express $\underset{n-m}{\overset{\prime}{B}}$ by the fluxional coefficients of $\underset{n-1}{B}$; after which we shall only have to substitute in equations (2) and (3).

Now it appears, from what has been shewn in this article, that

$$\underset{n-1}{\overset{\prime}{B}} = \left(\frac{\underset{n}{\dot{B}}}{\overset{\prime}{c}\cdot}\right);\ \underset{n-2}{\overset{\prime}{B}} = \left(\frac{\underset{n}{\dot{B}}}{\overset{\prime\prime}{c}\cdot}\right) = \left(\frac{\underset{n-1}{\dot{B}}}{\overset{\prime}{c}\cdot}\right);\ \underset{n-3}{\overset{\prime}{B}} = \left(\frac{\underset{n}{\dot{B}}}{\overset{\prime\prime\prime}{c}\cdot}\right) = \left(\frac{\underset{n-1}{\dot{B}}}{\overset{\prime\prime}{c}\cdot}\right)$$

$$= \left(\frac{\underset{n-2}{\dot{B}}}{\overset{\prime}{c}\cdot}\right) = \left(\frac{\underset{n-1}{\ddot{B}}}{\overset{\prime}{c}\cdot^2}\right);$$ (where by strokes put *under* a quan-

tity, I represent the reverse of the operations denoted by the strokes *over* it)* and, in general,

$$\underset{n-m}{\overset{\prime}{B}} = \left(\frac{\underset{n}{\dot{B}}}{\overset{\prime\prime\cdots m}{c}\cdot}\right) = \left(\frac{\underset{n-1}{\dot{B}}}{\overset{\prime\prime\cdots(m-1)}{c}\cdot}\right) =, \&c. \ldots(4); \quad \underset{n-m}{\overset{\prime}{B}} = \left(\frac{\overset{m-1}{\underset{n-1}{\dot{B}}}}{\overset{{}_{\prime}\,m-1}{c}\cdot}\right)_{\prime\prime\cdots\cdots(m-2)} \ldots\ldots(5).$$

It is evident that we might find many other relations between the B's and their fluxional coefficients; but those I have given seem the most useful.

4. By means of these equations, we may find $\underset{n}{B}$ from $\underset{n-1}{B}$ in several ways.

First Method. If we substitute, in equation (2), for $\underset{n-2}{\overset{\prime}{B}}$, $\left(\frac{\underset{n-1}{\dot{B}}}{\overset{\prime}{c}\cdot}\right)$; for $\underset{n-3}{\overset{\prime}{B}}$, $\left(\frac{\underset{n-1}{\dot{B}}}{\overset{\prime\prime}{c}\cdot}\right)$; for $\underset{n-4}{\overset{\prime}{B}}$, $\left(\frac{\underset{n-1}{\dot{B}}}{\overset{\prime\prime\prime}{c}\cdot}\right)$; .... for $\underset{n-n}{\overset{\prime}{B}}$, $\left(\frac{\underset{n-1}{\dot{B}}}{\overset{\prime\prime\cdots n(-1)}{c}\cdot}\right)$; which values are got from equation (4), we find

$$n\,\underset{n}{B} = n\,\overset{\prime\prime\cdots n}{c}\left(\frac{\underset{n-1}{\dot{B}}}{\overset{\prime\prime\cdots(n-1)}{c}\cdot}\right) + (n-1)\,\overset{\prime\prime\cdots(n-1)}{c}\left(\frac{\underset{n-1}{\dot{B}}}{\overset{\prime\prime\cdots(n-2)}{c}\cdot}\right) + \cdots\cdots\cdots$$

$$+ 2\,\overset{\prime\prime}{c}\left(\frac{\underset{n-1}{\dot{B}}}{\overset{\prime}{c}\cdot}\right) + \overset{\prime}{c}\left(\frac{\underset{n-1}{\dot{B}}}{\dot{c}}\right) \ldots\ldots (6).$$

This expression agrees with M. ARBOGAST's first method, and affords the following rule.

To find $\underset{n}{B}$ *from* $\underset{n-1}{B}$, *take the fluxion of the latter, with respect to* $c, \overset{\prime}{c}, \overset{\prime\prime}{c}$, *&c. and change generally* $\overset{\prime\prime\cdots m}{c}\cdot$ *into* $\overset{\prime\prime\cdots(m+1)}{c} \times \frac{m+1}{n}$.

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* Any number of strokes under a quantity will represent the depression of the fluxional coefficients of + (*c*) therein contained so many orders.

This rule is, however, more simple in the enunciation than the practice; on which account I proceed to a

5. *Second Method.* We might obtain one from equations (2) and (5), but, as it would be somewhat worse than the last, I omit it; and substitute in equation (3), the values of $\overset{\prime}{\dot{B}}_{n-2}$, $\overset{\prime}{\dot{B}}_{n-3}$, &c. given by (5). We find thus

$$\underset{n}{B} = \int\left(\frac{\dot{B}_{n-1}}{\dot{c}}\right)\overset{\prime}{c}^{\cdot} + \int\left(\frac{\dot{B}_{n-1}}{\overset{\prime}{c}^{\cdot}}\right)\overset{\prime\prime}{c}^{\cdot} + \int\left(\frac{\ddot{B}_{n-1}}{\overset{\prime}{c}^{2}}\right)_{\prime}\overset{\prime\prime\prime}{c}^{\cdot} + \int\left(\frac{\dddot{B}_{n-1}}{\overset{\prime}{c}^{\cdot 3}}\right)_{\prime\prime}\overset{\prime\prime\prime\prime}{c}^{\cdot} +, \&c. \ldots. (7),$$

or, if we consider $\underset{n-1}{B}$ under the form $\underset{n-1}{B} = \beta + \overset{(1)}{\beta}\overset{\prime}{c} + \overset{(2)}{\beta}\overset{\prime}{c}^{2} + \overset{(3)}{\beta}\overset{\prime}{c}^{3} +,$

$$\underset{n}{B} = \int \overset{\prime}{B}_{n-1}\overset{\prime}{c}^{\cdot} + 1\int \overset{(1)}{\beta}\overset{\prime\prime}{c}^{\cdot} + 1.2\int \underset{\prime}{\overset{(2)}{\beta}}\overset{\prime\prime\prime}{c}^{\cdot} + 1.2.3\int \underset{\prime\prime}{\overset{(3)}{\beta}}\overset{\prime\prime\prime\prime}{c}^{\cdot} +, \&c. \ldots\ldots (8)^{*}$$

Where any number of strokes under the β's denotes that the fluxional coefficients of $f\,(c)$ therein contained, must be depressed so many orders.

In this expression, we must neglect in $\overset{(2)}{\beta}$ all terms containing $\overset{\prime\prime}{c}$, in $\overset{(3)}{\beta}$ all those containing $\overset{\prime\prime}{c}$ or $\overset{\prime\prime\prime}{c}$, and so on. Let it be required, for an example, to find the coefficient of x^{7}, from that of x^{6} given in article 3. We shall have, after neglecting such terms as are above specified,

$$\overset{(1)}{\beta} = \overset{\prime\prime}{f}(c)\,\overset{\prime\prime\prime\prime}{c} + \overset{\prime\prime\prime}{f}(c)\,\overset{\prime\prime}{c}\,\overset{\prime\prime\prime}{c};\ \overset{(2)}{\beta} = \overset{\prime\prime\prime}{f}(c)\frac{\overset{\prime\prime\prime\prime}{c}}{2};\ \overset{(6)}{\beta} = \overset{\prime\prime\prime\prime\prime\prime}{f}(c)\,\frac{1}{2.3\,4\,5.6},$$

and by performing the operations indicated by equation (8), we find

$$\int \overset{\prime}{B}_{6}\,\overset{\prime}{c}^{\cdot} = \overset{\prime\prime}{f}(c)\,\overset{\prime}{c}\,\overset{\prime\prime\prime\prime\prime}{c} + \overset{\prime\prime\prime}{f}(c)\left|\begin{array}{l} \frac{\overset{\prime}{c}^{2}}{2}\overset{\prime\prime\prime\prime}{c} \\ +\overset{\prime}{c}\,\overset{\prime\prime}{c}\,\overset{\prime\prime\prime}{c} \\ +\overset{\prime}{c}\,\frac{\overset{\prime\prime\prime}{c}^{2}}{2} \end{array}\right. + \overset{\prime\prime\prime\prime}{f}(c)\left|\begin{array}{l} \frac{\overset{\prime}{c}^{3}}{2.3}\overset{\prime\prime\prime}{c} \\ +\frac{\overset{\prime}{c}^{2}}{2}\overset{\prime\prime}{c}\,\overset{\prime\prime\prime}{c} \\ +\overset{\prime}{c}\,\frac{\overset{\prime\prime}{c}^{3}}{2.3} \end{array}\right. + \overset{\prime\prime\prime\prime\prime}{f}(c)\left|\begin{array}{l} \frac{\overset{\prime}{c}^{4}}{2.3.4}\overset{\prime\prime\prime}{c} \\ +\frac{\overset{\prime}{c}^{3}}{2.3}\,\frac{\overset{\prime\prime}{c}^{2}}{2} \end{array}\right. + \overset{\prime\prime\ldots 6}{f}(c)\,\frac{\overset{\prime}{c}^{5}}{2.3.4.5}\overset{\prime\prime}{c} + \overset{\prime\prime\ldots 7}{f}(c)\,\frac{\overset{\prime}{c}^{7}}{2.3.4\ldots.7}$$

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* See Note III. at the end.

$$\int\overset{(1)}{\beta}\,\overset{\prime\prime}{c}{}^{\cdot} = \overset{\prime\prime}{f}(c)\,\overset{\prime\prime}{c}\,\overset{\prime\prime\prime\prime}{c} + \overset{\prime\prime\prime}{f}(c)\,\frac{\overset{\prime\prime}{c}{}^{2}}{2}\,\overset{\prime\prime\prime}{c};\quad 2\int\underset{\prime}{\overset{(2)}{\beta}}\,\overset{\prime\prime\prime}{c}{}^{\cdot} = \overset{\prime\prime}{f}(c)\,\overset{\prime\prime\prime}{c}\,\overset{\prime\prime\prime\prime}{c};$$

$2.3.4.5.6\int\underset{\prime\prime\prime\prime\prime}{\overset{(6)}{\beta}}\,\overset{\prime\prime\ldots7}{c}{}^{\cdot} = \overset{\prime}{f}(c)\,\overset{\prime\prime\ldots7}{c}$; these being added give $\underset{7}{B}$ the coefficient of x^7, which was required.

To find $\underset{n}{B}$ from equation (7) requires the use of both fluxions and fluents; in (8) we are without the fluxional process; but, in its place, have the trouble of observing the numeral coefficients of each term: there is, however, a way of avoiding the mention of fluents, and the necessity of paying attention to these coefficients. If we consider equation (3) and the mode of expansion derived from it, it will be evident, that whenever we have any power (as the mth) of $\overset{\prime}{c}$ or $\overset{\prime\prime}{c}$ or $\overset{\prime\prime\prime}{c}$, or &c. it must be divided by the product $2.3.4\ldots.m$. This follows from the manner of finding the fluent of such a quantity as $\overset{\prime}{c}{}^{r}\,\overset{\prime}{c}{}^{\cdot}$, and the consideration that we cannot arrive at $\overset{\prime}{c}{}^{m}$ without having passed through all the lower powers, and repeated the fluential process at each. Hence results the following rule; (where by $\overset{(2)}{\beta}$, $\overset{(3)}{\beta}$, &c. I mean these quantities after we have neglected in each of them the terms that have been already specified).

Omitting all the denominators, multiply $\underset{n-1}{\overset{\prime}{B}}$ by $\overset{\prime}{c}$; $\overset{(1)}{\beta}$ by $\overset{\prime\prime}{c}$; $\underset{\prime}{\overset{(2)}{\beta}}$ by $\overset{\prime\prime\prime}{c}$; $\underset{\prime\prime}{\overset{(3)}{\beta}}$ by $\overset{\prime\prime\prime\prime}{c}$; and so on: add these products together, and wherever there is any power of $\overset{\prime}{c}$, $\overset{\prime\prime}{c}$, &c. as the m:h, put the product $2.3.4\ldots.m$ for a denominator.

6. *Third Method.* If we substitute in equation (3) the

values of $\underset{n-2}{\overset{\prime}{\dot{B}}}$, $\underset{n-3}{\overset{\prime}{\dot{B}}}$, &c. given by (4), and it will become

$$\underset{n}{B} = \int \left(\frac{\underset{n-1}{\dot{B}}}{\dot{c}}\right) \overset{\prime}{c}\cdot + \int \left(\frac{\underset{n-1}{\dot{B}}}{\overset{\prime}{c}\cdot}\right) \overset{\prime\prime}{c}\cdot + \int \left(\frac{\underset{n-1}{\dot{B}}}{\overset{\prime\prime}{c}\cdot}\right) \overset{\prime\prime\prime}{c}\cdot + \int \left(\frac{\underset{n-1}{\dot{B}}}{\overset{\prime\prime\prime}{c}\cdot}\right)$$

$\overset{\prime\prime\prime\prime}{c}\cdot +$, &c. ...... (9),

where we must neglect, in the second term, every thing that contains $\overset{\prime}{c}$; in the third term every thing that contains $\overset{\prime}{c}$ or $\overset{\prime\prime}{c}$; and so on.

If we did not do this, we should have the same combinations of letters, frequently, more than once. We may, however, instead of proceeding according to the above given direction, omit the superfluous terms *at last;* and then the rule will be as follows:

To find $\underset{n}{B}$, *take the fluxion of* $\underset{n-1}{B}$ *with respect to* c, $\overset{\prime}{c}$, $\overset{\prime\prime}{c}$, *&c. and after changing, every where,* $\overset{\prime\prime\ldots m}{c}\cdot$ *into* $\overset{\prime\prime\ldots(m+1)}{c}\cdot$, *take the fluent with respect to this last; observing to keep only once the same combination of letters.*

But now let us consider, whether we cannot, by omitting to make certain of the letters vary, prevent the same combinations from being repeated.

First, if, in the term $P \times \left(\overset{\prime\prime\ldots(r-m)}{c}\right)^{n} \times \left(\overset{\prime\prime\ldots r}{c}\right)^{q}$, we make $\overset{\prime\prime\ldots(r-m)}{c}$ vary, according to the rule just now given, there results the combination $P \times \left(\overset{\prime\prime\ldots(r-m)}{c}\right)^{n-1} \times \overset{\prime\prime\ldots(r-m+1)}{c} \times \left(\overset{\prime\prime\ldots r}{c}\right)^{q}$ .... (α); but the same fluxional coefficient of $f(c)$ that is multiplied by $P \times \left(\overset{\prime\prime\ldots(r-m)}{c}\right)^{n} \times \left(\overset{\prime\prime\ldots r}{c}\right)^{q}$ will be also multiplied by $P \times \left(\overset{\prime\prime\ldots(r-m)}{c}\right)^{n-1}$

$\times \overset{''\ldots(r-m+1)}{c} \times \overset{''\ldots(r-1)}{c} \times \left(\overset{''\ldots r}{c}\right)^{q-1}$, (where $\overset{''\ldots(r-1)}{c}$ is either the last factor, with respect to the number of strokes, or the last but one, accordingly as q is equal to or greater than one); if we make this term vary, with respect to $\overset{''\ldots(r-1)}{c}$, we shall have the combination marked (α) over again.

Let us next consider when it will be necessary to make c vary. In the term $\overset{''\ldots n}{f}(c) \times \mathrm{P} \times \left(\overset{''\ldots m}{c}\right)^{p} \ldots\ldots (\beta)$, if we make c vary, there arises the combination $\overset{''\ldots(n+1)}{f(c)} \times \overset{\prime}{c} \times \mathrm{P} \times \left(\overset{''\ldots m}{c}\right)^{p}$; but the same coefficient (as $\underset{r}{\mathrm{B}}$) that contains (β), will also contain $\overset{''\ldots(n+1)}{f(c)} \times \overset{\prime}{c} \times \mathrm{P} \times \overset{''\ldots(m-1)}{c} \times \left(\overset{''\ldots m}{c}\right)^{p-1}$ which when we make it vary with respect to $\overset{''\ldots(m-1)}{c}$ gives also $\overset{''\ldots(n+1)}{f(c)} \times \overset{\prime}{c} \times \mathrm{P} \times \left(\overset{''\ldots m}{c}\right)^{p}$. Now $\overset{''\ldots(m-1)}{c}$ was here the last quantity or the last but one. We may then affirm, in general, that, if we make every term in $\underset{n-1}{\mathrm{B}}$ vary with respect to the last quantity, and the last but one also, when this immediately precedes the last, not in place only, but in the number of its strokes, we shall get all the terms we ought to have, any further variation only giving the same over again. From equation (9) we have then the following

Rule.

To find $\underset{n}{\mathrm{B}}$ *take the fluxion of* $\underset{n-1}{\mathrm{B}}$ *with respect to the last of the quantities* $c, \overset{\prime}{c}, \overset{\prime\prime}{c}$, *&c. in each term, and the last but one also, if it immediately precede the last in the number of strokes.*

Change every where $\overset{\prime\prime\ldots m}{c}\cdot$ *into* $\overset{\prime\prime\ldots(m+1)}{c}\cdot$ *and take the fluent with respect to this last.*

This is exactly the same rule as that given by M. ARBOGAST in p. 25 of his work.

7. The method pursued in this paper, has a remarkable advantage over M. ARBOGAST's in what he calls *inverse derivation*;* which I shall shew hereafter to be extremely useful in the expansion of double and triple, &c. multinomials. In the present case, of a simple one, we have, as was shewn at the end of Art. 3,

$$\mathrm{B}_{n-1}=\left(\frac{\dot{\mathrm{B}}_n}{\overset{\prime}{c}\cdot}\right)_1;\ \mathrm{B}_{n-2}=\left(\frac{\dot{\mathrm{B}}_n}{\overset{\prime\prime}{c}\cdot}\right)_1;\ \mathrm{B}_{n-3}=\left(\frac{\dot{\mathrm{B}}_n}{\overset{\prime\prime\prime}{c}\cdot}\right)_1;\ \ldots\ldots\mathrm{B}_{n-m}=\left(\frac{\dot{\mathrm{B}}_n}{\overset{\prime\prime\ldots m}{c}\cdot}\right)_1;$$

whence this

Rule.

To find B_{n-m}, *the coefficient of* x^{n-m}, *from* B_n *that of* x^n, *take the fluxional coefficient of the latter, with respect to* $\overset{\prime\prime\ldots m}{c}$, *and at the same time depress, to the next lower order, all the fluxional coefficients of* $f(c)$ *that are in* $\left(\frac{\dot{\mathrm{B}}_n}{\overset{\prime\prime\ldots m}{c}\cdot}\right)$.

Thus from the coefficient of x^6, which was found in Art. 3, we get

$$\mathrm{B}_5=\left(\frac{\dot{\mathrm{B}}_6}{\overset{\prime}{c}\cdot}\right)_1=\overset{\prime}{f}(c)\overset{\prime\prime\prime\prime\prime}{c}+\overset{\prime\prime}{f}(c)\left|\begin{array}{l}\overset{\prime}{c}\,\overset{\prime\prime\prime\prime}{c}\\+\overset{\prime\prime}{c}\,\overset{\prime\prime\prime}{c}\end{array}\right.+\overset{\prime\prime\prime}{f}(c)\left|\begin{array}{l}\frac{\overset{\prime}{c}{}^2}{2}\overset{\prime\prime\prime}{c}\\+\overset{\prime}{c}\frac{\overset{\prime\prime}{c}{}^2}{2}\end{array}\right.+\overset{\prime\prime\prime\prime}{f}(c)\frac{\overset{\prime}{c}{}^3}{2.3}\overset{\prime\prime}{c}+\overset{\prime\prime\prime\prime\prime}{f}(c)\frac{\overset{\prime}{c}{}^5}{2.3.4.5}$$

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* See Note III. at the end.

$$\underset{4}{\mathrm{B}}=\left(\frac{\dot{\mathrm{B}}_{6}}{\overset{\prime\prime}{c}}\right)_{1}=\overset{\prime}{f}(c)\overset{\prime\prime\prime\prime}{c}+\overset{\prime\prime}{f}(c)\left|\begin{array}{l}\overset{\prime}{c}\overset{\prime\prime\prime}{c}\\+\frac{\overset{\prime\prime}{c}^{2}}{2}\end{array}\right.+\overset{\prime\prime\prime}{f}(c)\frac{\overset{\prime}{c}^{2}}{2}\overset{\prime\prime}{c}+\overset{\prime\prime\prime\prime}{f}(c)\frac{\overset{\prime}{c}^{4}}{2\cdot3\,4}$$

&c. &c.

8. To complete the theory of the expansion of any function of a simple multinomial, there remains, for us to solve, the following

Problem.

It is required to find $\underset{n}{\mathrm{B}}$ *without knowing any of the coefficients that precede or follow it.*

It is, in the first place, evident enough, from what has been done, that

$$\underset{n}{\mathrm{B}}=\overset{\prime}{f}(c)\overset{\prime\prime\ldots n}{c}+\overset{\prime\prime}{f}(c)\overset{\prime\prime}{\psi}+\overset{\prime\prime\prime}{f}(c)\overset{\prime\prime\prime}{\psi}+\ldots\ldots\ldots+\overset{\prime\prime\ldots(m-1)}{f}(c)\overset{\prime\prime\ldots(m-1)}{\psi}$$

$$+\overset{\prime\prime\ldots m}{f}(c)\overset{\prime\prime\ldots m}{\psi}+\ldots\ \ldots\ldots+\overset{\prime\prime\ldots n}{f}(c)\frac{\overset{\prime n}{c}}{2\cdot3\cdot4\ldots\, n},$$

where $\overset{\prime\prime\ldots m}{\psi}$ consists (without considering the denominators) of all the combinations that can be formed of $\overset{\prime}{c}$, $\overset{\prime\prime}{c}$, $\overset{\prime\prime\prime}{c}$, &c. in which the sum of the strokes shall be n,* and the sum of the exponents m. But to form these combinations, for the higher powers, would not be very easy. It may not be amiss to inquire, therefore, for some regular method of immediately deriving $\overset{\prime\prime\ldots(m-1)}{\psi}$ from $\overset{\prime\prime\ldots m}{\psi}$; so that we may get all the ψ's successively, beginning with $\frac{\overset{\prime n}{c}}{2.3\ldots n}$ which multiplies $\overset{\prime\prime\ldots n}{f}(c)$.

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* I mean when the powers are expanded, as when $\overset{\prime}{c}^{3}$ is written $\overset{\prime}{c}\,\overset{\prime}{c}\,\overset{\prime}{c}$.

I shall take no notice of any numbers, which divide the different terms, till the end of the operation; having shewn, in Article 5, that it will be sufficient then to place the product $2.3.4\ldots\mu$ under every μth power.

There can be no difficulty in perceiving, that all the combinations in $\psi^{\prime\prime\ldots(m-1)}$ may be derived from those terms in $\psi^{\prime\prime\ldots m}$, that are multiplied by the powers of $\overset{\prime}{c}$, in the following manner. First, diminish the exponent of $\overset{\prime}{c}$ by one. Then, diminish the exponent of one of the other quantities by one, and multiply by the quantity that has the next greater number of strokes. For, if $\left(c^{\prime\prime\ldots r}\right)^{a} \times \mathrm{P} \times \left(c^{\prime\prime\ldots s}\right)^{b}$ be one of the combinations in $\psi^{\prime\prime\ldots(m-1)}$, there must necessarily be in $\psi^{\prime\prime\ldots m}$ the combination $\overset{\prime}{c} \times \left(c^{\prime\prime\ldots r}\right)^{a} \times \mathrm{P} \times c^{\prime\prime\ldots(s-1)} \times \left(c^{\prime\prime\ldots s}\right)^{b-1}$; and from this the former one is derived, in the manner above-mentioned, by taking away the $\overset{\prime}{c}$ and changing $c^{\prime\prime\ldots(s-1)}$ into $c^{\prime\prime\ldots s}$.

We will next see if there be any quantities that it would be superfluous to make vary. Let ${\overset{\prime}{c}}^{b} \times \mathrm{P} \times \left(c^{\prime\prime\ldots(p-q)}\right)^{r} \times \left(c^{\prime\prime\ldots p}\right)^{s}$ be a term of $\psi^{\prime\prime\ldots m}$, we find from it, by the prescribed operation, the two terms of $\psi^{\prime\prime\ldots(m-1)}$ ${\overset{\prime}{c}}^{b-1} \times \mathrm{P} \times \left(c^{\prime\prime\ldots(p-q)}\right)^{r} \times \left(c^{\prime\prime\ldots p}\right)^{s-1} \times c^{\prime\prime\ldots(p+1)}$ and ${\overset{\prime}{c}}^{b-1} \times \mathrm{P} \times \left(c^{\prime\prime\ldots(p-q)}\right)^{r-1} \times c^{\prime\prime\ldots(p-q+1)} \times \left(c^{\prime\prime\ldots p}\right)^{s} \ldots (\alpha)$; but $\psi^{\prime\prime\ldots m}$ will also have the combination ${\overset{\prime}{c}}^{b} \times \mathrm{P} \times \left(c^{\prime\prime\ldots(p-q)}\right)^{r-1} \times c^{\prime\prime\ldots(p-q+1)} \times c^{\prime\prime\ldots(p-1)} \times \left(c^{\prime\prime\ldots p}\right)^{s-1}$, from which the combination (α) may be got by diminishing by one the exponent of $\overset{\prime}{c}$ and changing $\overset{\prime\prime\ldots(p-1)}{c}$ into $\overset{\prime\prime\ldots p}{c}$. Now $\overset{\prime\prime\ldots(p-1)}{c}$ *is either the last quantity or the last but one in the order of the strokes.*

We can then have no difficulty in perceiving the truth of the following

*Rule.**

1st. *To find* $\overset{\prime\prime\ldots(m-1)}{\psi}$ *take no notice of any terms in* $\overset{\prime\prime\ldots m}{\psi}$ *but those that are multiplied by the powers of* $\overset{\prime}{c}$, *in all these diminish the exponent of* $\overset{\prime}{c}$ *by one; and omit the denominators.*

2dly. *Diminish the exponent of the last quantity, in these terms, by one; and multiply by the quantity that has the next greater number of strokes.*

3dly. *If the last quantity but one be that which immediately precedes the last in the number of strokes, make it vary in the same manner as was directed for the last.*

4thly. *All the combinations being thus formed, put the product* 2.3.4....μ *under every* μ*th power.*

The reader may compare this rule with that given by M. ARBOGAST, p. 36.

Suppose it were required to find B_{10}; we must begin with $\overset{\prime\prime\ldots10}{\psi} = \frac{\overset{\prime}{c}^{10}}{2.3.4\ldots10}$; from which is derived by the rule, $\overset{\prime\prime\ldots9}{\psi} = \frac{\overset{\prime}{c}^{8}}{2.3\ldots8} \times \overset{\prime\prime}{c}$, $\overset{\prime\prime\ldots8}{\psi} = \frac{\overset{\prime}{c}^{7}}{2.3\ldots7} \times \overset{\prime\prime\prime}{c} + \frac{\overset{\prime}{c}^{6}}{2.3\ldots6} \times \frac{\overset{\prime\prime}{c}^{2}}{2}$, and so on, whence

$$B_{10} = f(\overset{\prime\prime\ldots10}{c})\frac{\overset{\prime}{c}^{10}}{2.3.4\ldots10} + f(\overset{\prime\prime\ldots9}{c})\frac{\overset{\prime}{c}^{8}}{2.3.4\ldots8}\overset{\prime\prime}{c} + f(\overset{\prime\prime\ldots8}{c})\left\{\frac{\overset{\prime}{c}^{7}}{2.3.4\ldots7}\overset{\prime\prime\prime}{c} + \frac{\overset{\prime}{c}^{6}}{2.3.4\ldots6}\frac{\overset{\prime\prime}{c}^{2}}{2}\right\}$$

+, &c.

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* See Note I. at the end.

The succeeding terms are found with equal ease, I omit to find them only on account of the length of the calculation.

9. I shall now show that the same method may be successfully employed in more complicated cases; and, instead of dwelling on particular problems, shall proceed at once to *the expansion of any function of any functions of simple multinomials,*

$$\varphi\left\{\mathrm{F}\left(c+\overset{\prime}{c}x+\overset{\prime\prime}{c}x^2+\right), f\left(e+\overset{\prime}{e}x+\overset{\prime\prime}{e}x^2+\right), \text{\&c.}\right\}\ldots\ldots(\alpha).$$

If we consider $c+\overset{\prime}{c}x+$, $e+\overset{\prime}{e}x+$, &c. as binomials $c+y$, $e+z$, &c. the function (α), which may be, for the moment, represented by φ, will have for its fluxion, (y, z, &c. being made to vary)

$\left(\frac{\dot\varphi}{\dot y}\right)\dot y+\left(\frac{\dot\varphi}{\dot z}\right)\dot z+$ &c. $=\left(\frac{\dot\varphi}{\dot c}\right)\dot y+\left(\frac{\dot\varphi}{\dot e}\right)\dot z+$ &c.; and consequently $\int\left\{\left(\frac{\dot\varphi}{\dot c}\right)\dot y+\left(\frac{\dot\varphi}{\dot e}\right)\dot z+\text{\&c.}\right\}=\varphi=\varphi\left\{\mathrm{F}\left(c+\overset{\prime}{c}x+\overset{\prime\prime}{c}x^2+\right),\right.$ $\left.f\left(e+\overset{\prime}{e}x+\overset{\prime\prime}{e}x^2+\right), \text{\&c.}\right\}\ldots(\beta).$

If then we represent the expansion of the function (α) by the series

$$\mathrm{B}+\underset{1}{\mathrm{B}}x+\underset{2}{\mathrm{B}}x^2+\underset{3}{\mathrm{B}}x^3+\ldots\ldots+\underset{n}{\mathrm{B}}x^n+\ldots\ldots(\gamma)$$

and denote the fluxional coefficients, of the first order of $\mathrm{B}, \underset{1}{\mathrm{B}}, \underset{2}{\mathrm{B}}$, &c. with respect to c thus $\overset{c}{\mathrm{B}}, \underset{1}{\overset{c}{\mathrm{B}}}, \underset{2}{\overset{c}{\mathrm{B}}}$, &c.;

with respect to e thus $\overset{e}{\mathrm{B}}, \underset{1}{\overset{e}{\mathrm{B}}}, \underset{2}{\overset{e}{\mathrm{B}}}$, &c.;

&c. &c.

the equation marked (β) will become

$$\varphi\left\{\mathrm{F}\left(c+\overset{\prime}{c}x+\right), f\left(e+\overset{\prime}{e}x+\right), \text{\&c.}\right\}=\int\left\{\overset{c}{\mathrm{B}}+\underset{1}{\overset{c}{\mathrm{B}}}x+\underset{2}{\overset{c}{\mathrm{B}}}x^2+\right\}$$

$$(\overset{\prime}{c}\dot{x}+2\overset{\prime\prime}{c}x\dot{x}+)+\int\left\{\overset{e}{B}+\overset{e}{\underset{1}{B}}x+\overset{e}{\underset{2}{B}}x^{2}+\right\}(\overset{\prime}{e}\dot{x}+2\overset{\prime\prime}{e}x\dot{x}+)+\&c.$$

whence, after taking the fluents, and comparing the coefficients of the powers of x, with those of the same powers in (γ), we find

$$\underset{1}{B}=\overset{\prime}{c}\overset{c}{B}+\overset{\prime}{e}\overset{e}{B}+\&c.$$

$$\underset{2}{B}=\frac{2\overset{\prime\prime}{c}\overset{c}{B}+\overset{\prime}{c}\overset{c}{\underset{1}{B}}}{2}+\frac{2\overset{\prime\prime}{e}\overset{e}{B}+\overset{\prime}{e}\overset{e}{\underset{1}{B}}}{2}+\&c.$$

$$\underset{3}{B}=\frac{3\overset{\prime\prime\prime}{c}\overset{c}{B}+2\overset{\prime\prime}{c}\overset{c}{\underset{1}{B}}+\overset{\prime}{c}\overset{c}{\underset{2}{B}}}{3}+\frac{3\overset{\prime\prime\prime}{e}\overset{e}{B}+2\overset{\prime\prime}{e}\overset{e}{\underset{1}{B}}+\overset{\prime}{e}\overset{e}{\underset{2}{B}}}{3}+\&c.$$

. . . .

$$\underset{n}{B}=\frac{n\overset{\prime\prime\ldots n}{c}\overset{c}{B}+(n-1)\overset{\prime\prime\ldots(n-1)}{c}\overset{c}{\underset{1}{B}}+\ldots\ldots+2\overset{\prime\prime}{c}\overset{c}{\underset{n-2}{B}}+\overset{\prime}{c}\overset{c}{\underset{n-1}{B}}}{n}$$

$$+\frac{n\overset{\prime\prime\ldots n}{e}\overset{e}{B}+(n-1)\overset{\prime\prime\ldots(n-1)}{e}\overset{e}{\underset{1}{B}}+\ldots\ldots+2\overset{\prime\prime}{e}\overset{e}{\underset{n-2}{B}}+\overset{\prime}{e}\overset{e}{\underset{n-1}{B}}}{n}+\&c.\ \ldots\ldots\ (\delta).$$

But $B=\varphi\left\{F(c),f(e),\&c.\right\}$ whence all the coefficients are known.

10. This solution, however, gives no very expeditious way of actually expanding the function in question; particularly when we get to the higher powers: but by proceeding as in the second method, made use of for a function of one single multinomial, we find

$$\underset{n}{B}=\int\overset{c}{\underset{n-1}{\dot{B}}}\overset{\prime}{c}\cdot+\int\overset{c}{\underset{n-2}{\dot{B}}}\overset{\prime\prime}{c}\cdot+\int\overset{c}{\underset{n-3}{\dot{B}}}\overset{\prime\prime\prime}{c}\cdot+\ldots\ldots+\int\overset{e}{\underset{n-1}{\dot{B}}}\overset{\prime}{e}\cdot+\int\overset{e}{\underset{n-2}{\dot{B}}}\overset{\prime\prime}{e}\cdot+\int\overset{e}{\underset{n-3}{\dot{B}}}\overset{\prime\prime\prime}{e}\cdot+$$

$$\ldots\ldots+\&c.\ \ldots\ldots\ (\varepsilon)$$

where we must neglect in $\overset{c}{\underset{n-2}{B}}$ all terms which contain $\overset{\prime}{c}$; in $\overset{c}{\underset{n-3}{B}}$

all those which contain $\overset{\prime}{c}$ or $\overset{\prime\prime}{c}$; and so on. In $\underset{n-1}{\overset{e}{\mathrm{B}}}$ must be neglected all terms which contain any of the c's; in $\underset{n-2}{\overset{e}{\mathrm{B}}}$ these, and those also containing $\overset{\prime}{e}$; and, *in general, all those terms must be neglected, as we proceed, which contain any quantities whose fluxions have entered into the preceding terms.*

From the above equation is derived an easy mode of expansion, I shall give an example in the case of two functions; and shall represent, for brevity, $\varphi\left\{\mathrm{F}(c), f(e)\right\}$ by φ, and its fluxional coefficient of the $\overline{m+n}$th order (when the fluxion has been taken m times with respect to c and n times with respect to e) by $\overset{m,n}{\varphi}$.

We find here, $\mathrm{B}=\varphi$; $\underset{1}{\mathrm{B}}=\overset{1,0}{\varphi}\overset{\prime}{c}+\overset{0,1}{\varphi}\overset{\prime}{e}$;

$\underset{2}{\mathrm{B}}=\overset{1,0}{\varphi}\overset{\prime\prime}{c}+\overset{2,0}{\varphi}\frac{\overset{\prime}{c}^2}{2}+\overset{0,1}{\varphi}\overset{\prime\prime}{e}+\overset{0,2}{\varphi}\frac{\overset{\prime}{e}^2}{2}+\overset{1,1}{\varphi}\overset{\prime}{c}\overset{\prime}{e}$; but to explain more fully the manner of proceeding, let it be required to find $\underset{3}{\mathrm{B}}$ from the preceding coefficients. We have, after neglecting the specified terms,

$\int^{c}\underset{2}{\mathrm{B}}\overset{\prime}{c}=\overset{2,0}{\varphi}\overset{\prime}{c}\overset{\prime\prime}{c}+\overset{3,0}{\varphi}\frac{\overset{\prime}{c}^3}{2.3}+\overset{1,1}{\varphi}c\overset{\prime\prime}{c}+\overset{1.2}{\varphi}\overset{\prime}{c}\frac{\overset{\prime}{c}^2}{2}+\overset{2,1}{\varphi}\frac{\overset{\prime}{c}^2}{2}\overset{\prime}{e}$; $\int^{c}\underset{1}{\mathrm{B}}\overset{\prime\prime}{c}=\overset{1,1}{\varphi}\overset{\prime\prime}{c}\overset{\prime}{e}$

$\int^{c}\mathrm{B}\overset{\prime\prime\prime}{c}=\overset{1,0}{\varphi}\overset{\prime\prime\prime}{c}$; $\int^{e}\underset{2}{\mathrm{B}}\overset{\prime}{e}=\overset{0,2}{\varphi}\overset{\prime}{e}\overset{\prime\prime}{e}+\overset{0.3}{\varphi}\frac{\overset{\prime}{e}^3}{2.3}$; $\int^{e}\mathrm{B}\overset{\prime\prime\prime}{e}=\overset{0,1}{\varphi}\overset{\prime\prime\prime}{e}$; the sum of these gives

$$\underset{3}{\mathrm{B}}=\overset{1,0}{\varphi}\overset{\prime\prime\prime}{c}+\overset{2,0}{\varphi}\overset{\prime}{c}\overset{\prime\prime}{c}+\overset{3,0}{\varphi}\frac{\overset{\prime}{c}^3}{2.3}+\overset{0,1}{\varphi}\overset{\prime\prime\prime}{e}+\overset{0,2}{\varphi}\overset{\prime}{e}\overset{\prime\prime}{e}+\overset{0.3}{\varphi}\frac{\overset{\prime}{e}^3}{2.3}+\overset{1,1}{\varphi}(\overset{\prime}{c}\overset{\prime\prime}{e}+\overset{\prime\prime}{c}\overset{\prime}{e})$$
$$+\overset{1,2}{\varphi}\overset{\prime}{c}\frac{\overset{\prime}{e}^2}{2}+\overset{2,1}{\varphi}\frac{\overset{\prime}{c}^2}{2}\overset{\prime}{e}.$$

11. To get methods of deriving any coefficient from the one immediately preceding it, we must substitute, in (δ) and (ε), the values of $\overset{c}{\underset{n-m}{B}}$ and $\overset{e}{\underset{n-m}{B}}$ given by the following equations, which are similar to those found before, in the case of one multinomial, and marked (4) and (5).

$$\overset{c}{\underset{n-m}{B}} = \left(\frac{\underset{n}{\dot{B}}}{\overset{''\ldots m}{e}\,\cdot}\right) = \left(\frac{\underset{n-1}{\dot{B}}}{\overset{''\ldots(m-1)}{c}\,\cdot}\right);\ \overset{e}{\underset{n-m}{B}} = \left(\frac{\underset{n}{\dot{B}}}{\overset{''\ldots m}{e}\,\cdot}\right) = \left(\frac{\underset{n-1}{\dot{B}}}{\overset{''\ldots(m-1)}{e}\,\cdot}\right);$$

&c. .... (η)

$$\overset{c}{\underset{n-m}{B}} = \left(\frac{\overset{m-1}{\underset{n-1}{\dot{B}}}}{\overset{,m-1}{c}\,\cdot}\right)_{cc\ldots(m-2)};\ \overset{e}{\underset{n-m}{B}} = \left(\frac{\overset{m-1}{\underset{n-1}{\dot{B}}}}{\overset{,m-1}{e}\,\cdot}\right)_{ee\ldots(m-2)};\ \&c. \ldots(\theta)$$

where, by any number of c's or e's placed under a quantity, I represent the depression of the fluxional coefficients of φ, contained in that quantity, so many orders, with respect to c or e.

By combining equations (η) and (δ), we find

$$n\underset{n}{B} = n\overset{''\ldots n}{c}\left(\frac{\underset{n-1}{\dot{B}}}{\overset{''\ldots(n-1)}{e}\,\cdot}\right) + (n-1)\overset{''\ldots(n-1)}{c}\left(\frac{\underset{n-1}{\dot{B}}}{\overset{''\ldots(n-2)}{c}\,\cdot}\right) + \ldots\ldots + 2\overset{''}{c}\left(\frac{\underset{n-1}{\dot{B}}}{\overset{'}{c}\,\cdot}\right) + \overset{'}{c}\left(\frac{\underset{n-1}{\dot{B}}}{\dot{c}}\right) + n\overset{''\ldots n}{e}\left(\frac{\underset{n-1}{\dot{B}}}{\overset{''\ldots(n-1)}{e}\,\cdot}\right) + (n-1)\overset{''\ldots(n-1)}{e}\left(\frac{\underset{n-1}{\dot{B}}}{\overset{''\ldots(n-2)}{e}\,\cdot}\right) + \ldots + 2\overset{''}{e}\left(\frac{\underset{n-1}{\dot{B}}}{\overset{'}{e}\,\cdot}\right) + \overset{'}{e}\left(\frac{\underset{n-1}{\dot{B}}}{\dot{e}}\right) + \&c.$$

which in words is this: *To find* $\underset{n}{B}$, *take the fluxion of* $\underset{n-1}{B}$ *with respect to*

$c, \overset{\prime}{c}, \overset{\prime\prime}{c}$, *&c.* $e, \overset{\prime}{e}, \overset{\prime\prime}{e}$, *&c. &c. and change, every where,* $\overset{\prime\prime\ldots m}{c}{}^{\cdot}$ *into* $\overset{\prime\prime\ldots(m+1)}{c} \times \frac{m+1}{n}$, *and* $\overset{\prime\prime\ldots m}{e}{}^{\cdot}$ *into* $\overset{\prime\prime\ldots(m+1)}{e} \times \frac{m+1}{n}$, *&c.*

The reader may try this rule on the examples in the last article: I proceed to simpler methods for practice.

By combining equations (θ) and (ϵ), and considering B_{n-1} under the forms $B_{n-1} = \beta + \overset{(1)}{\beta}\,\overset{\prime}{c} + \overset{(2)}{\beta}\,\overset{\prime}{c}{}^{2} + \overset{(3)}{\beta}\,\overset{\prime}{c}{}^{3} +$, and $B_{n-1} = \Delta + \overset{(1)}{\Delta}\,\overset{\prime}{e} + \overset{(2)}{\Delta}\,\overset{\prime}{e}{}^{2} + \overset{(3)}{\Delta}\,\overset{\prime}{e}{}^{3} +$, and &c. we find $B_n = \int^{c} B_{n-1}\,\overset{\prime}{c}{}^{\cdot} + \int \overset{(1)}{\beta}\,\overset{\prime\prime}{c}{}^{\cdot} + 2\int \overset{(2)}{\beta}_{c}\,\overset{\prime\prime\prime}{c}{}^{\cdot} + 2.3\int \overset{(3)}{\beta}_{cc}\,\overset{\prime\prime\prime\prime}{c}{}^{\cdot} + \ldots\ldots + \int^{e} B_{n-1}\,\overset{\prime}{e}{}^{\cdot} + \int \overset{(1)}{\Delta}\,\overset{\prime\prime}{e}{}^{\cdot} + 2\int \overset{(2)}{\Delta}_{e}\,\overset{\prime\prime\prime}{e}{}^{\cdot} + 2.3\int \overset{(3)}{\Delta}_{ee}\,\overset{\prime\prime\prime\prime}{e}{}^{\cdot} + \ldots\ldots +$ &c. where in $\overset{(2)}{\beta}$ all terms must be neglected which contain $\overset{\prime\prime}{c}$; and in general, according the rule given in article 10, all terms must be neglected that contain any quantities whose fluxions have entered into the preceding terms.

By this method the expansion might be accomplished without difficulty, each term is found at once, and no reductions are necessary: the one which I am going to give is, however, much better, being, I conceive, the simplest possible.

12. By combining the equations (η) and (ε), there results

$$B_n = \int\left(\frac{\dot{B}_{n-1}}{\dot{c}}\right)\overset{\prime}{c}{}^{\cdot} + \int\left(\frac{\dot{B}_{n-1}}{\overset{\prime}{c}{}^{\cdot}}\right)\overset{\prime\prime}{c}{}^{\cdot} + \int\left(\frac{\dot{B}_{n-1}}{\overset{\prime\prime}{c}{}^{\cdot}}\right)\overset{\prime\prime\prime}{c}{}^{\cdot} + \ldots\ldots\ldots$$

$$+ \int\left(\frac{\dot{B}_{n-1}}{\dot{e}}\right)\overset{\prime}{e}{}^{\cdot} + \int\left(\frac{\dot{B}_{n-1}}{\overset{\prime}{e}{}^{\cdot}}\right)\overset{\prime\prime}{e}{}^{\cdot} + \int\left(\frac{\dot{B}_{n-1}}{\overset{\prime}{e}}\right)\overset{\prime\prime\prime}{e}{}^{\cdot} + \ldots +, \text{\&c.}$$

where we must observe to neglect certain terms, according to the directions so often given: and if we apply here all that was said in article 6, when we were considering a similar expression for a function of one multinomial, we easily get the following

Rule.

To find $\underset{n}{\mathrm{B}}$ *from* $\underset{n-1}{\mathrm{B}}$, *in the expansion of a function of any functions of the multinomials,* $c+\overset{\prime}{c}\,x+\overset{\prime\prime}{c}\,x^2+$ *and* $e+\overset{\prime}{c}\,x+\overset{\prime\prime}{e}\,x^2+$ *and* $d+\overset{\prime}{d}\,x+\overset{\prime\prime}{d}\,x^2+$ *and &c.*

1st. Consider only the c's, and take the fluxion of $\underset{n-1}{\mathrm{B}}$, *with respect to the last of them in each term; and the last but one also, if it immediately precede the last in the number of its strokes: change, every where,* $\overset{\prime\prime\ldots m}{c}\,\dot{}$ *into* $\overset{\prime\prime\ldots(m+1)}{c}\,\dot{}$, *and take the fluent of each term with respect to this last.*

2dly. Neglect all terms in $\underset{n-1}{\mathrm{B}}$ *which contain* $\overset{\prime}{c}$, $\overset{\prime}{c}$, $\overset{\prime\prime\prime}{c}$, *&c. and proceed, with the remaining ones, in the same manner with respect to the e's.*

3dly. Neglect all terms in $\underset{n-1}{\mathrm{B}}$ *which contain* $\overset{\prime}{c}$, $\overset{\prime\prime}{c}$, $\overset{\prime\prime\prime}{c}$, *&c.* $\overset{\prime}{e}$, $\overset{\prime\prime}{e}$, $\overset{\prime\prime\prime}{e}$, *&c. and proceed, with the remaining ones, in the same manner with respect to the d's.—And so on.*

Let it be required, in the case of two multinomials, to find $\underset{4}{\mathrm{B}}$ from $\underset{3}{\mathrm{B}}$ which is given in article 10. The first part of the rule gives

$$\overset{1,0}{\varphi}\,\overset{\prime\prime\prime\prime}{c}+\overset{2,0}{\varphi}\,\Big(\overset{\prime}{c}\,\overset{\prime\prime\prime}{c}+\frac{\overset{\prime\prime}{c}{}^2}{2}\Big)+\overset{3,0}{\varphi}\,\frac{\overset{\prime}{c}{}^2}{2}\,\overset{\prime\prime}{c}+\overset{4,0}{\varphi}\,\frac{\overset{\prime}{c}{}^4}{2.3.4}+\overset{1,1}{\varphi}\,\overset{\prime\prime}{c}\,\overset{\prime\prime}{e}+\overset{2,1}{\varphi}\,\frac{\overset{\prime}{c}{}^2}{2}\,\overset{\prime\prime}{e}+\overset{1,1}{\varphi}\,\overset{\prime\prime\prime}{c}\,\overset{\prime}{e}$$

$$+\overset{1,2}{\varphi}\,\overset{\prime\prime}{c}\,\frac{\overset{\prime}{c}^2}{2}+\overset{2,2}{\varphi}\,\frac{\overset{\prime}{c}^2}{2}\,\frac{\overset{\prime}{e}^2}{2}+\overset{2,1}{\varphi}\,\overset{\prime}{c}\,\overset{\prime\prime}{c}\,\overset{\prime}{e}+\overset{3,1}{\varphi}\,\frac{\overset{\prime}{c}^3}{2.3}\,\overset{\prime}{e}+\overset{1,1}{\varphi}\,\overset{\prime}{c}\,\overset{\prime\prime\prime}{e}+\overset{1,2}{\varphi}\,\overset{\prime}{c}\,\overset{\prime}{e}\,\overset{\prime\prime}{e}+\overset{1,3}{\varphi}\,\overset{\prime}{c}\,\frac{\overset{\prime}{e}^3}{2.3}.$$

The second part gives

$$\overset{0,1}{\varphi}\,\overset{\prime\prime\prime\prime}{e}+\overset{0,2}{\varphi}\,(\overset{\prime}{e}\,\overset{\prime\prime}{e}+\frac{\overset{\prime\prime}{e}^2}{2})+\overset{0,3}{\varphi}\,\frac{\overset{\prime}{e}^2}{2}\,\overset{\prime\prime}{e}+\overset{0,4}{\varphi}\,\frac{\overset{\prime}{e}^4}{2.3.4}.$$

The sum of these is B_4. As the number of multinomials adds nothing to the difficulty of expansion, according to this method, it is useless to give more examples.

13. Nor does the number of multinomials make any difference as to the facility of *inverse derivation;* which depends on the equation

$$B_{n-m}=\left(\frac{\dot{B}_n}{\overset{\ldots m}{c}\cdot}\right)_c$$

Thus from B_4, just now given, in the case of two multinomials, let it be required to find B_2; we have

$$B_2=B_{4-2}=\left(\frac{\dot{B}_4}{\overset{\prime\prime}{c}\cdot}\right)_c=\overset{1,0}{\varphi}\,\overset{\prime\prime}{c}+\overset{2,0}{\varphi}\,\frac{\overset{\prime}{c}^2}{2}+\overset{0,1}{\varphi}\,\overset{\prime\prime}{e}+\overset{0,2}{\varphi}\,\frac{\overset{\prime}{e}^2}{2}+\overset{1,1}{\varphi}\,\overset{\prime}{c}\,\overset{\prime}{e}.$$

14. There remains the important

Problem.

To find B_r *without knowing any of the other coefficients.*

It will be plain to any one, who in the least considers the methods that have been employed, that B_r must contain all the possible combinations of $\overset{\prime}{c}, \overset{\prime\prime}{c}, \overset{\prime\prime\prime}{c}$, &c. $\overset{\prime}{e}, \overset{\prime\prime}{e}, \overset{\prime\prime\prime}{e}$, &c. $\overset{\prime}{d}, \overset{\prime\prime}{d}, \overset{\prime\prime\prime}{d}$, &c. &c. that can be formed with this condition, that the number of strokes be r. Every mth power will be divided by the product

2.3.4....m: and the fluxional coefficient* $\overset{\alpha,\beta,\gamma,\&c.}{\varphi}$, that multiplies any term, will have for the left hand figure over it that number which is the sum of the exponents of the c's; for the next figure on the same side that number which is the sum of the exponents of the e's; for the third that which is the sum of the exponents of the d's; and so on.

The only difficulty then is to find these combinations (without the possibility of missing any, or the trouble of finding the same more than once) by some regular process of derivation.

A rule was given in Art. 8, when we were considering the similar problem in the case of one multinomial, for deriving all the combinations in $\underset{r}{\mathrm{B}}$, in which the sum of the strokes is r, from c^{r} as origin of derivation.

The same rule will apply here, but instead of the one origin $\overset{\prime}{c}{}^{r}$, we have, in the case of two multinomials, the origins

$$\overset{\prime}{c}{}^{r},\ \overset{\prime}{c}{}^{r-1}\,\overset{\prime}{e},\ \overset{\prime}{c}{}^{r-2}\,\overset{\prime}{e}{}^{2},\ \ldots\ldots\ldots\ \overset{\prime}{c}{}^{2}\,\overset{\prime}{e}{}^{r-2},\ \overset{\prime}{c}\,\overset{\prime}{e}{}^{r-1},\ \overset{\prime}{e}{}^{r}.$$

Let us consider any particular origin as $\overset{\prime}{c}{}^{n}\,\overset{\prime}{e}{}^{m}$. I denote the term derived immediately from $\overset{\prime}{c}{}^{n}$ (by the rule in Art. 8,) by $\Delta\overset{\prime}{c}{}^{n}$; and the terms derived from this last, from the same rule by $\Delta^{2}\overset{\prime}{c}{}^{n}$; those got from $\Delta^{2}\overset{\prime}{c}{}^{n}$ by $\Delta^{3}\overset{\prime}{c}{}^{n}$; and so on.

It is evident that all the possible combinations (of the kind we are seeking) derived from the origin $\overset{\prime}{c}^{n}\,\overset{\prime}{e}^{m}$, will be expressed by the product

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* $\overset{\alpha,\beta,\gamma,\&c.}{\varphi}$ represents the fluxional coefficient of $\varphi\left\{\mathrm{F}(c), f(e), \mathrm{V}(d), \&c.\right\}$ of the order $\alpha+\beta+\gamma+$, &c. where the fluxion has been taken α times with respect to c, β times with respect to e, and γ times with respect to d; and so on.

$$(\overset{\prime}{c}^{n} + \Delta\,\overset{\prime}{c}^{n} + \Delta^{2}\,\overset{\prime}{c}^{n} + \Delta^{3}\,\overset{\prime}{c}^{n} +)\,(\overset{\prime}{e}^{m} + \Delta\,\overset{\prime}{e}^{m} + \Delta^{2}\,\overset{\prime}{e}^{m} + \Delta^{3}\,\overset{\prime}{e}^{m} +),$$

where each derivation of $\overset{\prime}{e}^{m}$ is multiplied by every one of $\overset{\prime}{c}^{n}$, and conversely each one of $\overset{\prime}{c}^{n}$ by every one of $\overset{\prime}{e}^{m}$.

We have nothing to do then but to deduce the derivations from $\overset{\prime}{c}^{n}$ and $\overset{\prime}{e}^{m}$ by the rule in Art. 8.

Suppose that $\underset{5}{B}$ was the coefficient required, and that we wanted all the combinations arising from $\overset{\prime}{c}^{3}\,\overset{\prime}{e}^{2}$. We have here

$$\Delta\,\overset{\prime}{c}^{3} = \overset{\prime}{c}\,\overset{\prime\prime}{c};\ \Delta^{2}\,\overset{\prime}{c}^{3} = \overset{\prime\prime\prime}{c};\ \Delta^{3}\,\overset{\prime}{c}^{3} = o;\ \Delta\,\overset{\prime}{e}^{2} = \overset{\prime\prime}{e};\ \Delta^{2}\,\overset{\prime}{e}^{2} = o,$$

and, by substituting these values in the above product, we find all the combinations arising from the origin $\overset{\prime}{c}^{3}\,\overset{\prime}{e}^{2}$ to be $\overset{\prime}{c}^{3}\,\overset{\prime}{e}^{2} + \overset{\prime}{c}^{3}\,\overset{\prime\prime}{e} + \overset{\prime}{c}\,\overset{\prime\prime}{c}\,\overset{\prime}{e}^{2} + \overset{\prime\prime\prime}{c}\,\overset{\prime}{e}^{2} + \overset{\prime}{c}\,\overset{\prime\prime}{c}\,\overset{\prime\prime}{e} + \overset{\prime\prime\prime}{c}\,\overset{\prime\prime}{e}$.

It would be as well to write down the appropriate denominators to each combination as we proceed: and when we had treated all the origins of derivation in this manner, there would only remain to arrange the terms under their proper fluxional coefficients.*

---

* Instead of $\overset{\prime}{c}^{n}\,\overset{\prime}{e}^{m}$, I might have taken for origin of derivation $\overset{n,o}{\varphi}\,\overset{\prime}{c}^{n} \times \overset{o,m}{\varphi}\,\overset{\prime}{e}^{m}$; and after multiplying the factors

$$(\overset{n,o}{\varphi}\,\overset{\prime}{c}^{n} + \overset{n-1,o}{\varphi}.\Delta\,\overset{\prime}{c}^{n} + \overset{n-2,o}{\varphi}.\Delta^{2}\,\overset{\prime}{c}^{n} +)\,(\overset{o,m}{\varphi}\,\overset{\prime}{e}^{m} + \overset{o,m-1}{\varphi}.\Delta\,\overset{\prime}{e}^{m} + \overset{o,m-2}{\varphi}.\Delta^{2}\,\overset{\prime}{e}^{m} +)$$

have changed $\overset{\nu,o}{\varphi} \times \overset{o,\mu}{\varphi}$ into $\overset{\nu,\mu}{\varphi}$; but this would only give additional trouble without answering any useful end: it is sufficiently plain that the appropriate fluxional coefficient of $\Delta^{\nu}\,\overset{\prime}{c}^{n} \times \Delta^{\mu}\,\overset{\prime}{e}^{m}$ will be $\overset{n-\nu,m-\mu}{\varphi}$.

If the function under consideration contain three multinomials, the origins of derivation will be

$$\begin{array}{l} \acute{c}^{r}, \ \acute{c}^{r-1}\acute{e}, \ \acute{c}^{r-2}\acute{e}^{2}, \ \ldots\ldots\ldots \ \acute{c}^{2}\acute{e}^{r-2}, \ \acute{c}\,\acute{e}^{r-1}, \ \acute{e}^{r} \\ \acute{d} \times (\acute{c}^{r-1}, \ \acute{c}^{r-2}\acute{e}, \ \acute{c}^{r-3}\acute{e}^{2}, \ \ldots\ldots\ldots \ \acute{c}\,\acute{e}^{r-2}, \ \acute{e}^{r-1}) \\ \acute{d}^{2} \times (\acute{c}^{r-2}, \ \acute{c}^{r-3}\acute{e}, \ \acute{c}^{r-4}\acute{e}^{2}, \ \ldots\ldots\ldots \ \acute{e}^{r-2}) \\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ \acute{d}^{r-2} \times (\acute{c}^{2}, \ \acute{c}\,\acute{e}, \ \acute{e}^{2}) \\ \acute{d}^{r-1} \times (\acute{c}, \ \acute{e}) \\ \acute{d}^{r} \end{array}$$

and all the possible combinations derived from each particular origin as, $\acute{c}^{n}\acute{e}^{m}\acute{d}^{p}$, will be expressed by the product

$$(\acute{c}^{n} + \Delta\acute{c}^{n} + \Delta^{2}\acute{c}^{n} +, \text{\&c.}) \ (\acute{e}^{m} + \Delta\acute{e}^{m} + \Delta^{2}\acute{e}^{m} +, \text{\&c.}) \ (\acute{d}^{p} + \Delta\acute{d}^{p} + \Delta^{2}\acute{d}^{p} +, \text{\&c.})$$

The reader will easily extend the method, if necessary, to a greater number of multinomials.

As we have, in this manner, a certain, easy, and regular way of finding all the combinations in any coefficient B_{r}, the problem is completely solved.

I go on to *multinomials of higher kinds:* and, with M. ARBOGAST, shall call those multinomials of the *n*th order which are disposed according to the powers and products of *n* different letters x, y, z, &c.

15. After having so fully entered into particulars, in the preceding cases, there can be no difficulty in perceiving, that *a complete theory of derivation, for the expansion of any function of any number of functions of multinomials, whether they be simple,*

or double, or triple, &c. is contained in the following equations.

$$\underset{m,\,n,\,r,\,\&c.}{\mathrm{B}} = \Sigma.\int \overset{c}{\underset{m-\mu,\,n-\nu,\,r-\rho,\,\&c.}{\mathrm{B}}} \times \overset{\dot{c}}{\mu,\nu,\rho,\&c.} + \Sigma.\int \overset{e}{\underset{m-\mu,\,n-\nu,\,r-\rho,\&c.}{\mathrm{B}}}$$

$$\times\, \overset{\dot{c}}{\mu,\nu,\rho,}\,\&c. +,\ \&c. \ldots\ldots\ (\kappa)$$

$$\underset{m-\mu,\,n-\nu,\,r-\rho,\,\&c.}{\mathrm{B}} = \left(\frac{\underset{m,n,r,\&c.}{\dot{\mathrm{B}}}}{\underset{\mu,\,\nu,\,\rho,\,\&c.}{\dot{c}}}\right)_{c} = \&c. \ldots\ldots\ldots\ (\lambda),$$

where $\underset{m,\,n,\,r,\,\&c.}{\mathrm{B}}$ is the coefficient of $x^m\,y^n\,z^r$, &c. in the expansion; $\overset{c}{\mu,\,\nu,\,\rho,}$ &c. and $\overset{e}{\mu,\,\nu,\,\rho,}$ &c. &c. are the coefficients of $x^\mu\,y^\nu\,z^\rho$ &c. before expansion, under the signs of the functions.

The sign Σ, in the first formula, expresses the sum of all the terms that can be formed by taking for μ, ν, ρ, &c. all the whole numbers from o to m, n, r, &c. μ, ν, ρ, &c. must not however all equal nothing at the same time.

It is scarcely necessary to observe, that certain terms are understood to be neglected in equation (κ), according to the rule given in article 10, which is, that all terms in the B's must be neglected, as we proceed, which contain quantities whose fluxions enter into the preceding terms.

The above expressions if considered not only in themselves, but with respect to the formulas that are immediately deducible from their developement and combination, in the manner that will presently be shewn, appear to be the most general and important in this branch of analysis.

16. Let it be required to expand the double multinomial function

$$\varphi\left\{\begin{array}{l} c + c_{1,0}\,x + c_{2,0}\,x^2 + c_{3,0}\,x^3 + \\ + c_{0,1}\,y + c_{1,1}\,xy + c_{2,1}\,x^2y + \\ \qquad + c_{0,2}\,y^2 + c_{1,2}\,xy^2 + \\ \qquad\qquad + c_{0,3}\,y^3 + \end{array}\right\}\text{into the form}\quad \begin{array}{l} B + B_{1,0}\,x + B_{2,0}\,x^2 + B_{3,0}\,x^3 + \\ + B_{0,1}\,y + B_{1,1}\,xy + B_{2,1}\,x^2y + \\ \qquad + B_{0,2}\,y^2 + B_{1,2}\,xy^2 + \\ \qquad\qquad + B_{0,3}\,y^3 + \end{array}$$

Equation ($\varkappa$) is in this case reduced to $B_{m,n} = \Sigma.\int \overset{\prime}{B}_{m-\mu,n-\nu} \times \dot{c}_{\mu,\nu}$ which expanded is

$$(\mu)\ \ldots\ldots\ B_{m,n} = \int \overset{\prime}{B}_{m-1,n}\,\dot{c}_{1,0} + \int \overset{\prime}{B}_{m-2,n}\,\dot{c}_{2,0} + \ldots\ldots\ldots\ldots$$
$$+ \int \overset{\prime}{B}_{m,n-1}\,\dot{c}_{0,1} + \int \overset{\prime}{B}_{m-1,n-1}\,\dot{c}_{1,1} + \ldots\ldots\ldots\ldots$$
$$+ \int \overset{\prime}{B}_{m,n-2}\,\dot{c}_{0,2} + \ldots\ldots\ldots\ldots$$
$$+$$

By this formula, we find the first terms of the expansion to be

$$\begin{array}{l} \varphi(c) + \overset{\prime}{\varphi}(c)\,c_{1,0}\,x + \left.\begin{array}{l} \overset{\prime}{\varphi}(c)\,c_{2,0} \\ + \overset{\prime\prime}{\varphi}(c)\,\dfrac{(c_{1,0})^2}{2} \end{array}\right| x^2 + \\ + \overset{\prime}{\varphi}(c)\,c_{0,1}\,y + \left.\begin{array}{l} \overset{\prime}{\varphi}(c)\,c_{1,1} \\ + \overset{\prime\prime}{\varphi}(c)\,c_{1,0}\times c_{0,1} \end{array}\right| xy + \\ + \end{array}$$

Let it be required to find $B_{2,1}$. We have, after neglecting the terms specified,

$$\int\underset{1,1}{\overset{\prime}{B}}\underset{1,0}{\dot{c}}=\overset{\prime\prime}{\phi}(c)\underset{1,0}{c}\times\underset{1,1}{c}+\overset{\prime\prime\prime}{\phi}(c)\frac{\left(\underset{1,0}{c}\right)^2}{2}\times\underset{0,1}{c};\ \int\underset{2,0}{\overset{\prime}{B}}\underset{0,1}{\dot{c}}=\overset{\prime\prime}{\phi}(c)\underset{0,1}{c}\times\underset{2,0}{c};$$

$\int\overset{\prime}{B}\underset{2,1}{\dot{c}}=\overset{\prime}{\phi}(c)\underset{2,1}{c}$; and these added together give

$$\underset{2,1}{B}=\overset{\prime}{\phi}(c)\underset{2,1}{c}+\overset{\prime\prime}{\phi}(c)\left\{\underset{1,0}{c}\times\underset{1,1}{c}+\underset{0,1}{c}\times\underset{2,0}{c}\right\}+\overset{\prime\prime\prime}{\phi}(c)\frac{\left(\underset{1,0}{c}\right)^2}{2}\times\underset{0,1}{c}.$$

I wrote the terms separately, and then collected them, for the better explanation of the method; but this double labour is by no means necessary: the coefficients may be formed and written down at once, as quickly as can be wished.

17. A very little consideration will convince us, that the terms $\int\underset{m,n-1}{\overset{\prime}{B}}\underset{0,1}{\dot{c}};\ \int\underset{m,n-2}{\overset{\prime}{B}}\underset{0,2}{\dot{c}};\ \ldots\ldots\int\underset{m,n-r}{\overset{\prime}{B}}\underset{0,r}{\dot{c}}$ may be entirely left out of formula (μ), excepting when the term we search is of the form $\underset{0,m}{B}$, in which case it is reduced to the coefficient of y^m in the expansion of $\phi\,(c+\underset{0,1}{c}\,y+\underset{0,2}{c}\,y^2+)$.

If then we neglect these terms, and, in the remaining ones, put for $\underset{m-\mu,n-\nu}{\overset{\prime}{B}}$ its value $\left(\dfrac{\underset{m-1,n}{\dot{B}}}{\underset{\mu-1,\nu}{\dot{c}}}\right)$ given by (λ), equation (μ) will become

$$\begin{aligned}(\nu)\ldots\ldots\ \underset{m,n}{B}=&\int\left(\frac{\underset{m-1,n}{\dot{B}}}{\dot{c}}\right)\underset{1,0}{\dot{c}}+\int\left(\frac{\underset{m-1,n}{\dot{B}}}{\underset{1,0}{\dot{c}}}\right)\underset{2,0}{\dot{c}}+\ldots\ldots\ldots\ldots\\ &+\int\left(\frac{\underset{m-1,n}{\dot{B}}}{\underset{0,1}{\dot{c}}}\right)\underset{1,1}{\dot{c}}+\ldots\ldots\ldots\ldots\\ &\ldots\ldots\ldots\ldots\ldots\ldots\\ &\ldots\ldots\ldots\ldots+\int\left(\frac{\underset{m-1,n}{\dot{B}}}{\underset{\mu-1,\nu}{\dot{c}}}\right)\underset{\mu,\nu}{\dot{c}}+\ldots\end{aligned}$$

by means of which equation we may find $\underset{m,n}{B}$ from $\underset{m-1,n}{B}$. Here, as usual, we must omit, in the successive fluxional coefficients, all terms containing quantities whose fluxions have been factors in the preceding terms of the formula. Let us, for an example, find $\underset{3,1}{B}$ from $\underset{2,1}{B}$ given in the last article; we have

$$\int\left(\frac{\underset{2,1}{\dot{B}}}{\dot{c}}\right)\underset{1,0}{\dot{c}} = \overset{''}{\varphi}(c)\,\underset{1,0}{c}\times\underset{2,1}{c}+\overset{'''}{\varphi}(c)\left\{\frac{\left(\underset{1,0}{c}\right)^{2}}{2}\times\underset{1,1}{c}+\underset{1,0}{c}\times\underset{0,1}{c}\times\underset{2,0}{c}\right\}+\overset{''''}{\varphi}(c)\,\frac{\left(\underset{1,0}{c}\right)^{3}}{2.3}\times\underset{0,1}{c};\ \int\left(\frac{\underset{2,1}{\dot{B}}}{\underset{1,0}{\dot{c}}}\right)\underset{2,0}{\dot{c}}=\overset{''}{\varphi}\,\underset{1,1}{c}\times\underset{2,0}{c};\ \int\left(\frac{\underset{2,1}{\dot{B}}}{\underset{2,0}{\dot{c}}}\right)\underset{3,0}{\dot{c}}=\overset{''}{\varphi}(c)\,\underset{0,1}{c}\times\underset{3,0}{c};\ \int\left(\frac{\underset{2,1}{\dot{B}}}{\underset{2,1}{\dot{c}}}\right)\underset{3,1}{\dot{c}}=\overset{'}{\varphi}(c)\,\underset{3,1}{c};$$

whence

$$\underset{3,1}{B}=\overset{'}{\varphi}(c)\,\underset{3,1}{c}+\overset{''}{\varphi}(c)\left\{\underset{1,0}{c}\times\underset{2,1}{c}+\underset{1,1}{c}\times\underset{2,0}{c}+\underset{0,1}{c}\times\underset{3,0}{c}\right\}+\overset{'''}{\varphi}(c)\left\{\frac{\left(\underset{1,0}{c}\right)^{2}}{2}\times\underset{1,1}{c}+\underset{1,0}{c}\times\underset{0,1}{c}\times\underset{2,0}{c}\right\}+\overset{''''}{\varphi}(c)\,\frac{\left(\underset{1,0}{c}\right)^{3}}{2.3}\times\underset{0,1}{c}.$$

18. We may also derive from equation (ν) the following simple

Rule.

To find $\underset{m,n}{B}$, *take the fluxion of* $\underset{m-1,n}{B}$ *with respect to all the quantities; change, every where,* $\underset{\mu,\nu}{\dot{c}}$ *into* $\underset{\mu+1,\nu}{\dot{c}}$, *and take the fluent with respect to this last. The same terms must be kept only once.* c *is* $\underset{0,0}{c}$.

By this rule we frequently find the same terms more than once, which disadvantage is, however, more than compensated by its shortness, and the ease and simplicity of the process.

Let it be required from $\underset{0,3}{B}$ to find successively $\underset{1,3}{B}$, $\underset{2,3}{B}$, &c. We saw, when treating of a simple multinomial, that

$\underset{0,3}{B} = \varphi'(c)\,\underset{0,3}{c} + \varphi''(c)\,\underset{0,1}{c} \times \underset{0,2}{c} + \varphi'''(c)\,\dfrac{\left(\underset{0,1}{c}\right)^3}{2.3}$ whence, by the rule,

$\underset{1,3}{B} = \varphi'(c)\,\underset{1,3}{c} + \varphi''(c)\left\{\underset{1,0}{c} \times \underset{0,3}{c} + \underset{1,1}{c} \times \underset{0,2}{c} + \underset{0,1}{c} \times \underset{1,2}{c}\right\} + \varphi'''(c)\left\{\underset{1,0}{c} \times \underset{0,1}{c} \times \underset{0,2}{c} + \dfrac{\left(\underset{0,1}{c}\right)^2}{2} \times \underset{1,1}{c}\right\} + \varphi''''(c)\,\underset{1,0}{c} \times \dfrac{\left(\underset{0,1}{c}\right)^3}{2.3}$; $\underset{2,3}{B} = \varphi'(c)\,\underset{2,3}{c}$ $+ \varphi''(c)\left\{\underset{2,0}{c} \times \underset{0,3}{c} + \underset{1,0}{c} \times \underset{1,3}{c} + \underset{2,1}{c} \times \underset{0,2}{c} + \underset{1,1}{c} \times \underset{1,2}{c} + \underset{0,1}{c} \times \underset{2,2}{c}\right\}$ $+ \varphi'''(c)\left\{\dfrac{\left(\underset{1,0}{c}\right)^2}{2} \times \underset{0,3}{c} + \underset{1,0}{c} \times \underset{1,1}{c} \times \underset{0,2}{c} + \underset{1,0}{c} \times \underset{0,1}{c} \times \underset{1,2}{c} + \underset{2,0}{c} \times \underset{0,1}{c}\right.$ $\times \underset{0,2}{c} + \underset{0,1}{c}\,\dfrac{\left(\underset{1,1}{c}\right)^2}{2} + \left.\dfrac{\left(\underset{0,1}{c}\right)^2}{2} \times \underset{2,1}{c}\right\} + \varphi''''(c)\left\{\dfrac{\left(\underset{1,0}{c}\right)^2}{2} \times \underset{0,1}{c} \times \underset{0,2}{c} + \underset{1,0}{c}\right.$ $\times \dfrac{\left(\underset{0,1}{c}\right)^2}{2} \times \underset{1,1}{c} + \underset{2,0}{c} \times \left.\dfrac{\left(\underset{0,1}{c}\right)^3}{2.3}\right\} + \varphi'''''(c)\,\dfrac{\left(\underset{1,0}{c}\right)^2}{2} \times \dfrac{\left(\underset{0,1}{c}\right)^3}{2.3}$. It is not necessary to calculate all the coefficients we may want by *direct derivation ;* when we have got a few, in this manner, we may find the rest by the *inverse method* which is much easier. M. Arbogast has put the twenty-eight first terms in a table ;* of these there was need to calculate *only four directly*, as I shall show hereafter. But, to give an example of this inverse proceeding, let it be required to find $\underset{2,2}{B}$ from $\underset{2,3}{B}$ just now given.

Equation (λ) becomes in the present case $\underset{m-\mu,\,n-\nu}{B} = \left(\dfrac{\dot{\underset{m.n}{B}}}{\dot{\underset{\mu,\nu}{c}}}\right)_{\prime}$;

whence $\underset{2,2}{B} = \underset{2-0,\,3-1}{B} = \left(\dfrac{\dot{\underset{2,3}{B}}}{\dot{\underset{0,1}{c}}}\right)_{\prime} = \varphi'(c)\,\underset{2,2}{c} + \varphi''(c)\left\{\underset{1,0}{c} \times \underset{1,2}{c}\right.$

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* *Calc. des Deriv.* p. 127.

$$+ \underset{2,0}{c} \times \underset{0,2}{c} + \frac{\left(\underset{1,1}{c}\right)^2}{2} + \underset{0,1}{c} \times \underset{2,1}{c} \Big\} + \overset{\prime\prime\prime}{\varphi}(c) \Big\{ \frac{\left(\underset{1,0}{c}\right)^2}{2} \times \underset{0,2}{c} + \underset{1,0}{c} \times \underset{0,1}{c}$$

$$\times \underset{1,1}{c} + \underset{2,0}{c} \times \frac{\left(\underset{0,1}{c}\right)^2}{2} \Big\} + \overset{\prime\prime\prime\prime}{\varphi}(c) \frac{\left(\underset{1,0}{c}\right)^2}{2} \times \frac{\left(\underset{0,1}{c}\right)^2}{2}.$$

19. Instead of leaving out of equation (μ) the terms of this form $\int \underset{m,\,n-r}{\overset{\prime}{\dot{B}}} \; \underset{0,r}{\dot{c}}$ as we did in article 17, we might have omitted those of the form $\int \underset{m-r,\,n}{\overset{\prime}{\dot{B}}} \; \underset{r,0}{\dot{c}}$; in which case it would have become

$$\underset{m,n}{B} = \int \underset{m,\,n-1}{\overset{\prime}{\dot{B}}} \; \underset{0,1}{\dot{c}} + \int \underset{m-1,\,n-1}{\overset{\prime}{\dot{B}}} \; \underset{1,1}{\dot{c}} + \cdots\cdots\cdots$$

$$\int \underset{m,\,n-2}{\overset{\prime}{\dot{B}}} \; \underset{0,2}{\dot{c}} + \cdots\cdots\cdots$$

$$\cdots\cdots\cdots\cdots$$

$$\cdots\cdots\cdots + \int \underset{m-\mu,n-\nu}{\overset{\prime}{\dot{B}}} \; \underset{\mu,\nu}{\dot{c}} + \cdots\cdots$$

Here, if we put for $\underset{m-\mu,\,n-\nu}{\overset{\prime}{\dot{B}}}$ its value $\left(\dfrac{\underset{m+1,\,n-1}{\dot{B}}}{\underset{\mu+1,\,\nu-1}{\dot{c}}}\right)$ derived from equation (λ), there results

$$\underset{m,n}{B} = \int \left(\frac{\underset{m+1,\,n-1}{\dot{B}}}{\underset{1,0}{\dot{c}}}\right) \underset{0,1}{\dot{c}} + \int \left(\frac{\underset{m+1,\,n-1}{\dot{B}}}{\underset{2,0}{\dot{c}}}\right) \underset{1,1}{\dot{c}} + \cdots\cdots\cdots\cdots$$

$$+ \int \left(\frac{\underset{m+1,\,n-1}{\dot{B}}}{\underset{1,1}{\dot{c}}}\right) \underset{0,2}{\dot{c}} + \cdots\cdots\cdots\cdots$$

$$\cdots\cdots\cdots\cdots + \int \left(\frac{\underset{m+1,\,n-1}{\dot{B}}}{\underset{\mu+1,\,\nu-1}{\dot{c}}}\right) \underset{\mu,}{\dot{c}}$$

where, in every successive fluxional coefficient, certain terms are to be omitted, according to the usual rule.

Perhaps the simplest way of using this equation, although we shall frequently get the same combinations more than once, is by the following

Rule.

To find $B_{m,n}$ *take the fluxion of* $B_{m+1,\,n-1}$ *with respect to all the quantities, excepting* c, $c_{0,1}$, $c_{0,2}$, *&c.; change every where,* $\dot{c}_{\mu,\nu}$ *into* $\dot{c}_{\mu-1,\,\nu+1}$ *and take the fluent with respect to this last. The same terms must be kept only once.*

$B_{3,1}$ was found in article 17, from which we have, by this rule;

$$B_{2,2} = \overset{\prime}{\varphi}(c)\, c_{2,2} + \overset{\prime\prime}{\varphi}(c)\left\{ c_{0,1} \times c_{2,1} + c_{1,0} \times c_{1,2} + c_{0,2} \times c_{2,0} + \frac{(c_{1,1})^2}{2} \right\} + \overset{\prime\prime\prime}{\varphi}(c)\left\{ \frac{(c_{1,0})^2}{2} \times c_{0,2} + \frac{(c_{0,1})^2}{2} \times c_{2,0} + c_{1,0} \times c_{0,1} \times c_{1,1} \right\} + \overset{\prime\prime\prime\prime}{\varphi}(c)\,\frac{(c_{1,0})^2}{2} \times \frac{(c_{0,1})^2}{2}.$$

Suppose that, beginning with $B_{6,0}$, we had calculated in this manner $B_{5,1}$, $B_{4,2}$, $B_{3,3}$; from these may be found, with the greatest ease, *and without any more direct derivation*, the twenty-eight first terms. For from $B_{5,1}$, $B_{4,2}$ we find $B_{1,5}$, $B_{2,4}$ merely by changing, in the former, the numbers that are on the right hand of the commas (under the c's) to the left hand, and the reverse. All the other terms are found, by inverse derivation, from the equation $B_{m-\mu,\,n-\nu} = \left(\dfrac{\dot{B}_{m,n}}{\dot{c}_{\mu,\nu}}\right)_1$.

Problem.

20. *To find* $\underset{m,n}{\mathrm{B}}$ *immediately, without knowing any of the other coefficients.*

The coefficient of $x^m\,y^n$ will easily appear, from what has been shewn, to have the following form;

$$\underset{m,n}{\mathrm{B}} = \overset{\prime}{\varphi}(c)\,\underset{m,n}{c} + \overset{\prime\prime}{\varphi}(c)\,\overset{\prime\prime}{\psi} + \ldots\ldots + \overset{\prime\prime\ldots(r-1)}{\varphi}(c)\,\overset{\prime\prime\ldots(r-1)}{\psi} + \overset{\prime\prime\ldots r}{\varphi}(c)\,\overset{\prime\prime\ldots r}{\psi} + \ldots\ldots$$

$$+ \overset{\prime\prime\ldots(m+n)}{\varphi}(c) \times \frac{\left(\underset{1,0}{c}\right)^m}{2.3\ldots m} \times \frac{\left(\underset{0,1}{c}\right)^n}{2.3\ldots n}$$ where $\overset{\prime\prime\ldots r}{\psi}$ contains all the combinations that can be formed of the c's (after c or $\underset{0,0}{c}$) in which the sum of the bottom figures, on the left of the commas, is m; the sum of those on the right n: and the number of factors r. Moreover every power as the mth will be divided by $2.3\ldots m$.

And the reader, who considers how the similar problem was solved, in the case of a simple multinomial, will have no difficulty in perceiving the reason of the following very simple

Rule.

To find $\overset{\prime\prime\ldots(r-1)}{\psi}$ *from* $\overset{\prime\prime\ldots r}{\psi}$, 1*st. take the fluxional coefficient, of the latter, with respect to* $\underset{1,0}{c}$; *and, of this fluxional coefficient, take the fluxion with respect to all the quantities; change generally* $\underset{\mu,\nu}{\dot{c}}$ *into* $\underset{\mu+1,\nu}{\dot{c}}$ *and take the fluent with respect to this last.*

2*dly. Any terms in* $\overset{\prime\prime\ldots r}{\psi}$ *of the form* $\frac{\left(\underset{1,0}{c}\right)^p}{2.3\ldots p} \times \frac{\left(\underset{0,1}{c}\right)^q}{2.3\ldots q} \times \frac{\left(\underset{0,s}{c}\right)^t}{2.3\ldots t} \times \frac{\left(\underset{0,u}{c}\right)^v}{2.3\ldots v} \times$ &c. *where, except in* $\underset{1,0}{c}$, *all the figures are on the right of the commas, will require, besides, the following process. Take the fluxional coefficient with respect to* $\underset{0,1}{c}$ *and, of this fluxional coefficient, take the* *fluxion with respect to all the quantities but $\underset{1,0}{c}$; change generally $\underset{0,\mu}{\dot{c}}$ into $\underset{0,\mu+1}{\dot{c}}$ and take the fluent with respect to this last. The same terms must be kept only once.*

By this rule, we find $\underset{m,n}{\mathrm{B}}$ beginning with $\overset{\prime\prime\ldots(m+n)}{\varphi}(c) \times \frac{\left(\underset{1,0}{c}\right)^m}{2\,3\ldots m} \times \frac{\left(\underset{0,1}{c}\right)^n}{2.3\ldots n}$ for origin of derivation: the reader may compare it with that given by M. Arbogast at p. 113 of his work.

21. If the function to be expanded contains functions of many double multinomials, all the formulas, and rules, that have been given for one, may be extended to this case, by means of equations ($\varkappa$) and (λ); in the same manner as a like extension was made in treating of simple multinomials.

Thus, from the method of finding $\underset{m,n}{\mathrm{B}}$ given in article 19, we get the following

Rule.

To find $\underset{m,n}{\mathrm{B}}$ from $\underset{m+1,\,n-1}{\mathrm{B}}$ in the expansion of any function of any functions of the double multinomials

$$\begin{array}{lll} c + \underset{1,0}{c}\,x + ; & e + \underset{1,0}{e}\,x + ; & d + \underset{1,0}{d}\,x + ; \ \&c. \\ \quad + \underset{0,1}{c}\,y + & \quad + \underset{0,1}{e}\,y + & \quad + \underset{0,1}{d}\,y + \\ \quad + & \quad + & \quad + \end{array}$$

1*st. Consider only the c's, and take the fluxion of $\underset{m+1,\,n-1}{\mathrm{B}}$ with respect to all of them except c, $\underset{0,1}{c}$, $\underset{0,2}{c}$, &c.; and proceed exactly in the same manner as was directed for one double multinomial in article* 19.

2*dly. Neglect all the terms in $\underset{m+1,\,n-1}{\mathrm{B}}$ which contain any of the c's but c; and proceed, with the remaining terms, in the same manner with respect to the e's.*

3dly. Neglect all terms in $\underset{m+1,\,n-1}{B}$ *which contain any of the c's or e's except c and e; and proceed, with the remaining terms, in the same manner with respect to the d's. And so on according to the number of multinomials.*

The sum of the terms, thus obtained, will give $\underset{m,n}{B}$.

It is scarcely necessary to observe that, when we have got a few of the higher terms, by this rule, the preceding ones may be found from the equation

$$\underset{m-\mu,\,n-\nu}{B} = \left(\frac{\underset{m,n}{\dot{B}}}{\underset{\mu,\nu}{\dot{c}}}\right)_c$$

as in the case of one double multinomial.

To find any coefficient, without a knowledge of the rest, when the function contains more than one double multinomial, we must combine the rule in the last article, with what was shewn in article 14.*

22. Thus we have a complete and simple theory of the expansion of functions of double multinomials; and from equations ($\varkappa$) and (λ) a precisely similar theory may be derived for multinomials of higher kinds.

But it is wholly unnecessary to enter into further details; we are able, without any more trouble, to see what must be the solution of the following

General Problem.

It is required, in the expansion of any function of a multinomial of any kind, to find $\underset{m,\,n,\,r,\,s,\,t,\,\&c.}{B}$ *the coefficient of* $x^m\, y^n\, z^r\, u^s\, v^t$ *&c. from* $\underset{m+1,\,n-1,\,r,\,s,\,t,\,\&c.}{B}$ *that of* $x^{m+1}\, y^{n-1}\, z^r\, u^s\, v^t$ *&c.*

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* See Note II.

Rule.

Take the fluxion of the latter with respect to all the quantities except c, $\underset{0,\mu}{c}$, $\underset{0,\mu,\nu}{c}$, $\underset{0,0,\rho}{c}$, *&c that have nothing on the left hand of the first comma; change generally* $\underset{\mu,\nu,\rho,\&c.}{\dot{c}}$ *into* $\underset{\mu-1,\nu+1,\rho,\&c.}{\dot{c}}$ *and take the fluent with respect to this last. The same terms must be kept only once.*

The extension of this to any number of multinomials is exactly the same as the similar extension, for double multinomials, in the last article.

Second General Problem.

23. *It is required, in the case of the last problem, to find* $\underset{m,n,r,s,t,\&c.}{B}$ *without a knowledge of any other coefficient.*

This will be accomplished if we can find $\overset{''\ldots(r-1)}{\psi}$ *which multiplies* $\overset{''\ldots(r-1)}{\varphi}(c)$ *from* $\overset{''\ldots r}{\psi}$ *which multiplies* $\overset{''\ldots r}{\varphi}(c)$.

Rule.

1*st. Take the fluxional coefficient of* $\overset{''\ldots r}{\psi}$ *with respect to* $\underset{1,0,0,0,0,\&c.}{c}$, *and of this fluxional coefficient take the fluxion with respect to all the quantities; change generally* $\underset{\mu,\nu,\rho,\&c.}{\dot{c}}$ *into* $\underset{\mu+1,\nu,\rho,\&c.}{\dot{c}}$ *and take the fluent with respect to this last.*

2*dly. If there be any terms in* $\overset{''\ldots r}{\psi}$ *in which the unit under* $\underset{1,0,0,0,0,\&c.}{c}$, *if it be one of the factors, is the only left hand figure, they will require a further process.*

Take the fluxional coefficient with respect to $\underset{0,1,0,0,0,\&c.}{c}$, *and of this take the fluxion with respect to all the quantities, except*

$\underset{1,0,0,0,0,\&c.}{c}$; *change generally* $\underset{0,\mu,\nu,\rho,\&c.}{\dot{c}}$ *into* $\underset{0,\mu+1,\nu,\rho,\&c.}{\dot{c}}$ *and take the fluent.*

3dly. Any terms, in $\overset{''\ldots r}{\psi}$, *in which the units under* $\underset{1,0,0,0,0,\&c.}{c}$ *and* $\underset{0,1,0,0,0,\&c.}{c}$, *if they are amongst the factors, are the only figures in the first and second left hand places, will require a still further process. Take the fluxional coefficient with respect to* $\underset{0,0,1,0,0,\&c.}{c}$, *and of this take the fluxion with respect to all the quantities except* $\underset{1,0,0,0,0,\&c.}{c}$ *and* $\underset{0,1,0,0,0,\&c.}{c}$: *change generally* $\underset{0,0,\mu,\nu,\rho,\&c.}{\dot{c}}$ *into* $\underset{0,0,\mu+1,\nu,\rho,\&c.}{\dot{c}}$ *and take the fluent.*

The rule will proceed in this manner, till it contains n *parts if the multinomial be of the* n*th order. The terms arising from all these parts must be added, and the same terms kept only once.*

24. In treating multinomials of higher kinds, I have given rules by which certain terms are frequently found more than once: this was done for the sake of simplicity, and that the precepts might be easily retained in the memory; but was by no means a matter of necessity; for rules might without difficulty have been formed (as from equations (ν) and (ξ) for a double multinomial) by which no superfluous terms would have been found.

25. It will not be an improper termination of this paper, to state what are the peculiar advantages of the method pursued in it.

To many, I have no doubt, its brevity will be a recommendation; and that it requires no notation different from that in common use.

For though I have represented some of the fluxional coefficients in an unusual manner, as $\left(\frac{\underset{n-1}{\dddot{\mathrm{B}}}}{\overset{\prime}{c}{}^{2}.\dot{c}}\right)$ by $\left(\frac{\underset{n-1}{\ddot{\mathrm{B}}}}{\overset{\prime}{c}{}^{2}}\right)^{c}$ the doing so was not necessary; but it appeared advisable to make a distinction between the taking the fluxion with respect to c, and the same operation with respect to $\overset{\prime}{c}$, $\overset{\prime\prime}{c}$, &c. which enter into the coefficients in a manner different from the first.

The uniformity of the procedure is such, that, when we have arrived at the rules for one simple multinomial, a person of any skill in this kind of inquiry might easily divine those for the more difficult cases. But the most important circumstance is the perfection given to *inverse derivation*, and the facility with which we may, by that means, find any large number of terms in the expansion of the higher kinds of multinomials, as has been shewn in article 18 and 19.

The last advantage I shall notice is, that the same rules of derivation serve equally for the expansion of a function of one or of a thousand multinomials: whereas, from M. ARBOGAST's methods, it would not, I imagine, be very easy to give a rule in words for the expansion of a function of five or six.

NOTES.

Note I. The rule in article 8 may be differently enunciated thus.

To find $\overset{''\ldots(m-1)}{\psi}$ *take the fluxional coefficient of* $\overset{''\ldots m}{\psi}$ *with respect to* $\overset{\prime}{c}$; *and of this fluxional coefficient take the fluxion with respect to the last quantities; change generally* $\overset{''\ldots m}{c}$ *into* $\overset{''\ldots(m+1)}{c}$ *and take the fluent with respect to this last.*

2*dly. If the last quantity but one be that which precedes the last in the number of strokes make it vary in the same manner and take the fluent.*

This is simpler than the rule in article 8, and more conformable to the mode of expression made use of in other parts of the paper.

Note II. In looking back on what I have written, I am apprehensive it may be thought that I have affected too great brevity in the last paragraph of article 21. That the reader may have no difficulty, the following problem is added, to illustrate what was said in the passage alluded to.

Problem.

To find at once $\underset{m,n}{\mathrm{B}}$ *in the expansion of a function of two functions of double multinomials.*

It is plain that $\underset{m,n}{\mathrm{B}}$ must contain all the possible combinations of *c*'s and *e*'s (see the notation of article 21) that can be formed with this condition; that the number of left hand strokes be *m*; the number of right hand strokes *n*. Every *r*th power must be divided by the product 2.3.4...*r*. And the fluxional coefficient $\overset{\alpha,\beta}{\varphi}$, that multiplies each term, will have, for the left hand figure and over it, the sum of the exponents of the *c*'s in that term; for the right hand figure β the sum of the exponents of the *e*'s.

Now to get all the combinations of the kind mentioned above, with their proper divisors, we must plainly take, for origins of derivation, all the terms of the following product, when actually multiplied.

$$\left\{\frac{\left(\underset{1,0}{c}\right)^m}{2.3\ldots m}+\frac{\left(\underset{1,0}{c}\right)^{m-1}}{2.3\ldots(m-1)}\times\underset{1,0}{e}+\frac{\left(\underset{1,0}{c}\right)^{m-2}}{2.3\ldots(m-2)}\times\frac{\left(\underset{1,0}{e}\right)^2}{2}+\ldots\ldots+\frac{\left(\underset{1,0}{e}\right)^m}{2.3\ldots m}\right\}$$

multiplied by $\left\{\frac{\left(\underset{0,1}{c}\right)^n}{2.3\ldots n}+\frac{\left(\underset{0,1}{c}\right)^{n-1}}{2.3\ldots(n-1)}\times\underset{0,1}{e}+\frac{\left(\underset{0,1}{c}\right)^{n-2}}{2.3\ldots(n-2)}\times\frac{\left(\underset{0,1}{e}\right)^2}{2}+\ldots\ldots+\frac{\left(\underset{0,1}{e}\right)^n}{2.3\ldots n}\right\}$

Suppose any one of these origins to be

$$\frac{\left({c \atop 1,0}\right)^r}{2.3\,..r} \times \frac{\left({c \atop 0,1}\right)^s}{2.3\ldots s} \times \frac{\left({e \atop 1,0}\right)^t}{2.3\ldots t} \times \frac{\left({e \atop 0,1}\right)^u}{2.3\ldots u} \ldots\ldots\ldots\ldots (\mathrm{A}).$$

Let $\triangle$, $\triangle^2$, $\triangle^3$, &c. represent the successive derivations made according to the rule in article 20. It is plain that all the terms got from the origin of derivation (A) will be expressed by the product

$$\left\{ \frac{\left({c \atop 1,0}\right)^r}{2.3\ldots r} \times \frac{\left({c \atop 0,1}\right)^s}{2.3\ldots s} + \triangle \left\{ \frac{\left({c \atop 1,0}\right)^r}{2.3\ldots r} \times \frac{\left({c \atop 0,1}\right)^s}{2.3\,..s} \right\} + \triangle^2 \left\{ \frac{\left({c \atop 1,0}\right)^r}{2.3\,..r} \times \frac{\left({c \atop 0.1}\right)^s}{2.3\ldots s} \right\} + \&c. \right\}$$

multiplied by

$$\left\{ \frac{\left({e \atop 1,0}\right)^t}{2.3\ldots t} \times \frac{\left({e \atop 0,1}\right)^u}{2.3\ldots u} + \triangle \left\{ \frac{\left({e \atop 1,0}\right)^t}{2.3\ldots t} \times \frac{\left({e \atop c,1}\right)^u}{2.3\ldots u} \right\} + \triangle^2 \left\{ \frac{\left({e \atop 1,0}\right)^t}{2.3\ldots t} \times \frac{\left({e \atop 0,1}\right)^u}{2.3\ldots u} \right\} + \&c. \right\}.$$

In this manner may the terms be derived from all the origins; after which we have only to arrange them under their appropriate fluxional coefficients.

If we wanted to find immediately ${\mathrm{B} \atop m,\, n,\, r,\, \&c.}$ in a function of two multinomials of a still higher kind, the method would be exactly similar.

Note III. In the preceding pages, I have considered the expansion of multinomial functions *generally*; and abstained from giving particular examples, that the paper might not be extended to an unreasonable length. There are, however, some cases, —*when the function is a whole positive power*—which require a separate notice. The method of direct derivation given in article 5, and a similar one at the end of article 11 will here fail: this indeed is of no consequence, as the rules in article 6 and 12 are both easier than the former, and applicable to every case But it will be necessary to give new methods of inverse derivation; for if we consider those in the paper, in article 7 for example, it will easily appear, that though they are true generally for the mth power, the case is very different when we give to this letter the particular values 1, 2, 3, &c. The reason of which is that the fluxional coefficients of $f\,(c)$, after the first, or the second, or the third, &c. vanish; and these functions may be said not improperly, when compared with the general form, to give *defective expansions*; any rules, therefore, which depend on the *depression* of the fluxional coefficients of $f\,(c)$ will be of no use here.

The following very extensive rule is the reverse of that, for direct derivation, in article 12. It agrees, in its *simplest case*, with that of M. Arbogast in his article 36.

Rule.

To find B_{n-1} from B_n in a function of any functions of the multinomials $c + \overset{\prime}{c}x + \overset{\prime\prime}{c}x^2 +$, $e + \overset{\prime}{e}x + \overset{\prime\prime}{e}x^2 +$, $d + \overset{\prime}{d}x + \overset{\prime\prime}{d}x^2 +$, &c. 1st. Consider only those terms, in B_n, which contain some of the quantities $\overset{\prime}{c}, \overset{\prime\prime}{c}, \overset{\prime\prime\prime}{c}$, &c.; reject all the terms in which the last of these letters are raised to a higher than the first power: reject also *(if there be more than one multinomial)* such terms as contain none of the above mentioned quantities but the first power of $\overset{\prime}{c}$. Change, generally, in each remaining term, the last of the c's as $\overset{\prime\prime\prime\ldots m}{c}$ into $\overset{\prime\prime\ldots(m-1)}{c}\cdot$ and take the fluent with respect to this quantity.

2dly. Neglecting those terms, in B_n, into which $\overset{\prime}{c}, \overset{\prime\prime}{c}, \overset{\prime\prime\prime}{c}$, &c. enter, consider those, of the remainder, which contain $\overset{\prime}{e}, \overset{\prime\prime}{e}, \overset{\prime\prime\prime}{e}$, &c. rejecting all those terms in which the last of the e's are raised to a higher than the first power. Those terms must also be rejected *(if there be more than two multinomials)* which contain none of the e's but the first power of $\overset{\prime}{e}$. Change generally, in the remaining terms, the last of the e's as $\overset{\prime\prime\ldots m}{e}$ into $\overset{\prime\prime\ldots(m-1)}{e}\cdot$ and take the fluent, with respect to this last.

3dly. Neglecting the terms into which $\overset{\prime}{c}, \overset{\prime\prime}{c}, \overset{\prime\prime\prime}{c}$, &c. $\overset{\prime}{e}, \overset{\prime\prime}{e}, \overset{\prime\prime\prime}{e}$, &c. enter, consider those, of the remainder, which contain $\overset{\prime}{d}, \overset{\prime\prime}{d}, \overset{\prime\prime\prime}{d}$, &c. and proceed as before.—And so on.

This rule has no difficulty, whatever may be the number of multinomials.

The words in italics, were inserted to make the rule include the finding of B_1 from B; they are of no use when n is greater than one.

Similar rules for multinomials of higher orders are formed with equal ease; being the reverse of those that have been given for direct derivation.

IV. *On a Case of nervous Affection cured by Pressure of the Carotids; with some physiological Remarks. By* C. H. Parry, *M. D. F. R. S.*

Read 20th December, 1810.

OBSERVING that the ROYAL SOCIETY, of which I have the honour to be a Member, occasionally receives communications illustrative of the laws of animal life, which are indeed the most important branch of physics, I take the liberty of calling their attention to a case, confirming a principle which I long ago published, and which, I believe, had never till then been remarked by pathologists.

About the year 1786, I began to attend a young lady, who laboured under repeated and violent attacks, either of head-ach, vertigo, mania, dyspnoea, convulsions, or other symptoms usually denominated nervous. This case I described at large to the Medical Society of London, who published it in their Memoirs, in the year 1788. Long meditation on the circumstances of the case, led me to conclude, that all the symptoms arose from a violent impulse of blood into the vessels of the brain; whence I inferred, that as the chief canals conveying this blood were the carotid arteries, it might perhaps be possible to intercept a considerable part of it so impelled, and thus remove those symptoms which were the supposed effect of that inordinate influx. With this view, I compressed with my thumb one or both carotids, and uniformly found all the symptoms removed by that process. Those circumstances of rapidity or intensity of thought, which constituted delirium, immediately ceased, and gave place to other trains of a healthy kind; head-ach and vertigo were removed, and a stop was put to convulsions, which the united strength of three or four attendants had before been insufficient to counteract.

That this extraordinary effect was not that of mere pressure, operating as a sort of counteracting stimulus, was evident: for the salutary effect was exactly proportioned to the actual pressure of the carotid itself, and did not take place at all, if, in consequence of a wrong direction either to the right or left, the carotid escaped the effects of the operation.

This view of the order of phœnomena was, in reality, very conformable to the known laws of the animal œconomy. It is admitted, that a certain momentum of the circulating blood in the brain, is necessary to the due performance of the functions of that organ. Reduce the momentum, and you not only impair those functions, but, if the reduction go to a certain degree, you bring on syncope, in which they are for a time suspended. On the other hand, in nervous affections, the sensibility and other functions of the brain are unduly increased; and what can be more natural than to attribute this effect to the contrary cause, or excessive momentum in the vessels of the brain? If, however, this analogical reasoning has any force in ascertaining the principle, I must acknowledge that it did not occur to me till twenty years afterwards, when a great number of direct experiments had appeared to me clearly to demonstrate the fact.

From various cases of this kind, I beg leave to select one which occurred to me in the month of January, 1805.

Mrs. T. aged 51, two years and a half beyond a certain critical period of female life, a widow, mother of two children, thin, and of a middle size, had been habitually free from gout, rheumatism, hœmorrhoids, eruptions, and all other disorders, except those usually called nervous, and occasional colds, one of which, about two years and a half before, had been accompanied with considerable cough, and had still left some shortness of breathing, affecting her only when she used strong muscular exertion, as in walking up stairs, or up hill.

In February 1803, after sitting for a considerable time in a room without a fire, in very severe weather, she was so much chilled as to feel, according to her own expression, " as if her " blood within was cold." In order to warm herself, she walked briskly for a considerable time about the house, but ineffectually. The coldness continued for several hours, during which she was seized with a numbness or sleepiness of her left side, together with a momentary deafness, but no privation or hebetude of the other senses, or pain or giddiness of the head. After the deafness had subsided, she became preternaturally sensible to sound in the ear of the affected side, and felt a sort of rushing or tingling in the fingers of the left hand, which led her to conclude that " the blood went " too forcibly there."

Though the coldness went off, what she called numbness still continued, but without the least diminution of the power of motion in the side affected. In about six weeks, the numbness extended itself to the right side.

Among various ineffectual remedies for these complaints, blisters were applied to the back, and the inside of the left arm above the elbow. The former drew well. The latter inflamed without discharging: so that a poultice of bread and milk was put on the blistered part. After this period, the muscles of the humerus began to feel as if contracted and stiff; and these sensations gradually spread themselves to the neck and head, and all across the body, so as to make it uncomfortable for her to lie on either side, though there was no inability of motion.

She now began to be affected with violent occasional flushings of her face and head, which occurred even while her feet and legs were cold, together with a rushing noise in the back of the head, especially in hot weather, or from any of those causes which usually produce the feelings of heat.

It is difficult to give intelligible names to sensations of a new and uncommon kind. That, which this lady denominated numbness, diminished neither the motion nor the sensibility of the parts affected. It was more a perception of tightness and constriction, in which the susceptibility of feeling in the parts was in fact increased; and the skin of the extremities was so tender, that the cold air produced a sense of uneasiness, the finest flannel or worsted felt disagreeably coarse, and the attempt to stick a pin with her fingers caused intolerable pain.

In the month of September 1803, not long after the application of the blisters, she experienced in certain parts of the left arm and thigh, that sensation of twitching which is vulgarly called the "life blood," and which soon extended itself to the right side. Shortly afterwards, she began to perceive an actual vibration or starting up of certain portions of the flexor muscles of the fore-arm, and of the deltoid on the left side; not so, however, as to move the arm or hand.

This disorder had continued with little variation to the period of my first visit. The vibrations constantly existed while the arm was in the common posture, the fore-arm and hand leaning on the lap. If the arm were stretched strongly downwards, the vibration of the flexors ceased, but those of the deltoid continued. The arm being strongly extended forwards, all ceased; but returned as soon as the muscles were relaxed. The vibrations were of different degrees of frequency, and at pretty regular intervals, usually about 80 in a minute. They were increased in frequency and force by any thing which agitated or heated the patient, and were always worse after dinner than after breakfast. The pulse in the radial artery was 80 in a minute, and rather hard. That in the carotids was very full and strong; and each carotid appeared to be unusually dilated for about half an inch in length, the adjacent portions above and below being much smaller, and of the natural size. I much regret that I find in my notes of this case, no inquiry whether there was any coincidence between the systoles of the heart, and the muscular vibrations. The patient's feet were usually cold, and her head and face hot. The feeling in her limbs was much as I have above described, except that the sensibility was somewhat less acute than it had been, and she complained of a tightness all over her head, as if it had been bound with a close night-cap. Her sleep was usually sound on first going to bed, but afterwards, for the most part, interrupted by dreaming. Bowels generally costive: appetite moderate: no flatulency or indigestion: tongue slightly furred, without thirst: urine variable, but generally pale.

The late Mr. GEORGE CROOK, surgeon, was present while I made these examinations; and when we afterwards conversed together, I remarked to him, that if my theory of the usual cause of spasmodic or nervous affections were well founded, I should probably be able to suppress or restrain these muscular vibrations of the left arm, by compressing the carotid artery on the opposite or right side; while little effect might perhaps be produced, by compressing the carotid of the side affected. The event was exactly conformable to my expectation. Strong pressure on the right carotid uniformly stopped all the vibrations, while that on the left had no apparent influence. I may add that these experiments were afterwards, at my request, repeated on this lady in London by Dr. BAILLIE, and, as he informed me in a letter, with a similar result.

It is perfectly well known to many of the learned Members of this Society, that irritations of the brain, when of moderate force, usually exhibit their effects on the nerves or muscles of the opposite side of the body; and in the case before us, it is difficult to understand how the suspension of these automatic motions could have been produced by this pressure of the opposite carotid, in any other way than by the interruption of the excessive flow of blood through a vessel morbidly dilated; in consequence of which interruption, the undue irritation of the brain was removed, and the muscular fibres permitted to resume their usual state of rest.

From these and many other similar facts, I am disposed to conclude, that irritation of the brain, from undue impulse of blood, is the common though not the only cause of spasmodic and nervous affections; and I can with the most precise regard to truth add, that a mode of practice, conformable to this principle has enabled me, during more than twenty years, to cure a vast number of such maladies, which had resisted the usual means.

An investigation of all the modifications of the principle itself, and of its numerous relations to Therapeutics, would be inconsistent with the views of the Royal Society, and must be reserved for another place.

Bath, Dec. 8, 1810.

V. *On the Non-existence of Sugar in the Blood of Persons labouring under Diabetes Mellitus. In a Letter to* Alexander Marcet, *M. D. F. R. S. from* William Hyde Wollaston, *M. D. Sec. R. S.*

Read January 24, 1811.

My dear Sir,

In reply to your inquiry respecting my experiments upon the non-existence of sugar in the serum of diabetic persons, which I have mentioned to you at different periods, I am really ashamed to reflect how long I have suffered them to remain neglected, when I consider their tendency to elucidate a curious point of physiological research.

My first endeavours to detect sugar in the serum of the blood were made soon after perusing the second edition of Dr. Rollo's Treatise on the Diabetes (which was published in 1798,) at the request of Dr. Baillie, who was so obliging as to furnish me with various specimens of diabetic blood and serum for this purpose.

The other set of experiments which I made with reference to the same question were not thought of till the following year. The inquiry was then left unfinished, and I never resumed it; for as I soon after* relinquished the practice of physic, I desisted in a great measure from prosecuting any inquiries connected with medicine.

However, since so much of this subject as is strictly physiological, relating to the natural course of circulating fluids, and more especially so much of the investigation as is conducted by chemical means, is within the range of those pursuits which are generally interesting to the Royal Society, I will endeavour to give you as distinct an account as I am able of the progress of my own experiments, requesting that you will in return state, more fully than you have hitherto done, the result of that further step in the inquiry which you took at my suggestion, and if it is agreeable to you, we will without delay make a joint communication of our researches to the Society.

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* In 1800.

Although Dr. ROLLO had been assisted in the chemical part of his inquiry by the well known talents of Mr. CRUICKSHANK, it appears that they "had not been so fortunate as to obtain "a sufficient quantity of serum for chemical experiment;"[*] and were unable fully to satisfy themselves by the taste or by other means which they could employ, concerning the existence or non-existence of sugar in the blood of persons labouring under diabetes; but nevertheless they were persuaded of its presence.

For the purpose of forming some judgment on this question, Mr. CRUICKSHANK made trial of the quantities of oxalic acid that could be formed from serum or from blood in their natural state, and from the same serum or blood after the addition of a certain proportion of sugar; and from the difference perceptible in these trials, he formed a probable conjecture respecting the presence or absence of sugar in the serum of diabetic persons.

This method, it is evident, is liable to a two-fold objection: first, that an excess of other ingredients beside sugar will cause an increase of the quantity of oxalic acid formed, and secondly, that slight variations in the process for forming oxalic acid will unavoidably occasion differences in the result.

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[*] ROLLO on Diabetes, p. 408.

The method which I employed appears to me capable of detecting much smaller quantities of such an ingredient, for though it might not enable us to distinguish exactly the nature of any small quantity that may be discovered, still the mere question of absence or presence admits of determination with great precision.

For this purpose I investigated, in the first place, how the albuminous part of healthy serum could be most completely coagulated, and by what appearances the presence of sugar that had been added to it would be most easily discerned.

When heat alone had been employed for the coagulation of serum, to which water had been added, that which exsuded from it was still found to contain a portion of albumen dissolved in it, and if this were allowed to remain, any saccharine matter which might be present would be disguised, and could not with certainty be detected.

I found, however, that this residuum of coagulable matter might be altogether prevented by the addition of a small quantity of dilute acid to the serum before coagulation.* To six drams of serum I added half a dram of muriatic acid previously diluted with one dram and a half of water, and immersed the phial containing them in boiling water during four minutes. The coagulation was thus rendered complete. In the course of a few hours a dram or more of water exsudes from serum that has been so coagulated. If a drop of this water be evaporated, the salts which it contains are found to crystallize, so that the form of the crystals may be easily distinguished; they are principally common salt.

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* I presumed that this portion of albumen was retained in solution by the alkali redundant in serum, and added the acid for the purpose of neutralizing it.

If any portion of saccharine matter has been added to the serum previous to coagulation, the crystallization of the salts is impeded, or wholly prevented, according to the quantity of sugar present.

If the quantity added does not exceed two grains and a half to the ounce, the crystallization is not prevented; but even this small quantity is perceptible by a degree of blackness that appears after evaporation: occasioned, as I suppose, by the action of a small excess of acid on the sugar.

If five grains have been added, the crystallization is very imperfect, and soon disappears in a moist air by deliquescence of the sugar. The blackness is also deeper than in the former case.

By addition of ten grains to the ounce, the crystallization of the salts is entirely prevented, and the degree of blackness, and disposition to deliquesce are of course more manifest than with smaller quantities.*

As I was aware that the sugar obtained from diabetic urine is a different substance from common sugar (approaching more nearly to the sugar of figs), I had the precaution to repeat the same series of experiments upon serum, to which I made corresponding additions of dry sugar, that I had formerly extracted from the urine of a person who voided it in considerable quantity; and I found the effects to be perfectly similar in every respect.

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* In the annexed Plate are represented the degrees of blackness of the drop occasioned by adding one, two, three, and four grains of sugar to six drachms of serum.

As a further test of the absence or presence of sugar, I found it convenient to add a little nitric acid to the salts that remained after crystallization of the drop. If the serum has been successfully coagulated without any addition of sugar, the addition of nitric acid merely converts the muriatic salts into nitrates, and nitrate of soda is seen to crystallize without foam or blackness. But when sugar has been added, a white foam rises round the margin of the drop, and if further heat be applied, it becomes black in proportion to the quantity of sugar present.

Such are the appearances when the proportions have been duly adjusted, and the proper heat for coagulation applied. I must own, however, that I could not always succeed to my satisfaction at the time when these experiments were conducted, and I am inclined to ascribe occasional failures to having used more muriatic acid than was really necessary, which by excess of heat might redissolve a part of the coagulated albumen, and thence occasion appearances, which, without careful discrimination, might be ascribed to sugar.

After having, by this course of experiment, satisfied myself as to the phenomena exhibited by serum in its natural state, and the effects of any small additions of sugar, I then proceeded to the examination of such specimens of diabetic blood or of serum, as I was able to procure.

The first which I examined was a portion of blood that had been taken from a person whose urine had been analysed, and found to contain sugar. This blood had been dried, when fresh, by a gentle heat, so as not to coagulate the serum. After being reduced to powder, it was mixed with water, in order that every thing which remained soluble might be ex-

Phil. Trans. MDCCCXI. *Plate* I. *p.* 101.

tracted. A little muriatic acid was then added, and sufficient heat applied for coagulation of the albumen. The water that separated after coagulation was found to contain the salts of the blood, but no trace whatever of sugar.

A second specimen of dried blood, that had been ascertained to be diabetic on the same evidence as the preceding, was examined in a similar manner, with the same result, as no appearance of sugar could be discerned.

In a third instance, I had some serum from the blood of a person whose urine had been tasted, and found " *very sweet.*" (I had no opportunity of procuring any of this urine for analysis). After a portion of this serum had been coagulated, with the addition of the usual proportion of muriatic acid, there was no appearance whatever of sugar. But when three grains of diabetic sugar had been added to another ounce of the same serum, the presence of this quantity was manifest by the same process.

I had also a fourth opportunity of examining serum of a person whose urine contained so much saccharine matter, that an ounce of it yielded, by evaporation, thirty-six grains of extract. In this instance I was not so successful in my experiment; for, though I was satisfied that no sugar was present, there certainly was a degree of blackness, which might have been occasioned by about one grain and a half of sugar in the ounce of serum. But this black matter appeared not to be sugar: it was more easily dried than sugar: it was not fusible by heat as sugar is: and its refractive power* was too great for that of sugar.

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* The method by which this was tried has since that time been described in the Philosophical Transactions for 1802.

I unfortunately had no opportunity of repeating the experiment on a second portion of the same serum, having inconsiderately employed it for other experiments, and coagulated it at the same time with the former.

In the next experiment I added half a dram of the urine of the same person to six drams of the serum and with a due proportion of diluted muriatic acid coagulated as before. Although the quantity of extract added did not exceed $\frac{36}{16}$, or two grains and a quarter of extract, the difference was very manifest by the darkness of the colour and the defective crystallization of the salts.

To the remaining quantity of the serum I had added twice the former proportion of the urine, and found that this quantity did not wholly prevent the crystallization of the salts during the evaporation of the drop.

The result of these trials was such, as to satisfy me that the serum in this instance contained no perceptible quantity of sugar, or at least that the water separable from the coagulated serum did not contain one-thirtieth part of that proportion which I had found in the urine of the same person.

In order to account for the presence of sugar in the urine, we must consequently either suppose a power in the kidneys of forming this new product by secretion, which does not seem to accord with the proper office of that organ; or, if we suppose the sugar to be formed in the stomach by a process of imperfect assimilation, we must then admit the existence of some channel of conveyance from the stomach to the bladder, without passing through the general system of blood-vessels. That some such channel does exist, Dr. DARWIN* endeavoured to ascertain, by giving large doses of nitre, which he could perceive to pass with the urine, but could not detect in its passage through the blood; and he imagined the channel by which it was conveyed to be the absorbent system, upon the supposition that they might admit of a retrograde motion of their contents.

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* Account of the retrograde Motion of the absorbent Vessels, by CHARLES DARWIN.

Without adopting the theory of Dr. DARWIN, it did appear to me that the fact deserved to be ascertained by some test more decisive than nitre, and I conceived that if prussiate of potash could be taken with safety, its presence would be discerned by means of a solution of iron in as small proportion as almost any known chemical test. Upon trial of this salt, I found that a solution of it might be taken without the least inconvenience, and that in less than one hour and a half the urine became perceptibly impregnated, and continued so to the fifth or sixth hour, although the quantity taken had not amounted to more than three grains of the salt.

After a few previous trials of the period when the principal impregnation of the urine might be expected, and when the presence of the prussiate (if it existed in the blood) might with most reason be presumed to occur, a healthy person about thirty-four years of age was induced to take a dose corresponding to three grains and a half of the dry salt, and to repeat it every hour to the third time. The urine being examined every half hour, was found in two hours to be tinged, and to afford a deep blue at the end of four hours. Blood was then taken from the arm, and the coagulum, after it had formed, was allowed to contract, so that the serum might be fully separated. The presence of the prussiate was then endeavoured to be discovered by means of a solution of iron, but without effect; and as I thought that the redundant alkali (which had been ascertained to prevail in this serum) might tend to prevent the appearance of the precipitate, I added a small quantity of dilute acid; but still I could not discern that any degree of blueness was occasioned by it.

This experiment having been repeated a second time with the same result, seemed to me nearly conclusive with respect to the existence of some passage, by which substances certainly known to be in the stomach may find their way to the bladder without being mixed with the general mass of circulating fluids.

Being desirous of ascertaining whether the prussiate could be discovered in any other secretions, I have repeatedly examined my saliva, at times when the urine has manifested a very strong blue, by adding solution of iron, but I could at no time perceive the saliva to be tinged.

I have also, during a severe cold, accompanied with profuse running of water from the nose, made a similar examination of this discharge, but have not been able to perceive any trace of the prussic acid.

It was nearly in this state that I left the inquiry at the period I have mentioned, and I do not remember to have made any other experiments, when I requested your assistance in making trial of the serum that is secreted in consequence of the application of a blister. Your report upon the result of your experiments, in addition to those which I have above related, nearly satisfied me as to the existence of some unknown channel of conveyance by which substances may reach the bladder.

With respect to Dr. Darwin's conception of a retrograde action of the absorbents, it is so strongly opposed by the known structure of that system of vessels, that I believe few persons will admit it to be in any degree probable.

Since we have become acquainted with the surprising chemical effects of the lowest states of electricity, I have been inclined to hope that we might from that source derive some explanation of such phenomena. But though I have referred* secretion in general to the agency of the electric power with which the nerves appear to be indued, and am thereby reconciled to the secretion of acid urine, from blood that is known to be alkaline, which before that time seemed highly paradoxical, and although the transfer of the prussiate of potash, of sugar, or of other substances may equally be effected by the same power as acting cause, still the channel through which they are conveyed remains to be discovered by direct experiment.

I have, indeed, conjectured that, by examining the blood in the abdominal vessels, or contents of the lacteals, it might be possible to detect them *in transitu;* but I have not been inclined to make such experiments on living animals, as would perhaps throw light upon the subject.

I remain, Dear Sir,
with great regard,
yours very truly,

W. H. WOLLASTON.

January 1, 1811.

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* Philosophical Magazine for June 1809.

Reply of Dr. MARCET *on the same Subject.*

Russell Square, January 8, 1811.

MY DEAR SIR,

I AM much gratified to find that you have at last been induced to communicate to the Royal Society your curious inquiry respecting the state of the blood in diabetes. I was anxious that the specious hypothesis of the presence of sugar in diabetic blood, which had been sanctioned by the authority of Dr. ROLLO and Mr. CRUICKSHANK, and which I had myself urged in support of their theory, fourteen years ago, in an inaugural publication, should no longer obtain an undue weight amongst physiological inquirers.

With regard to the experiments which I tried at your request some years ago, with a view to ascertain whether prussiat of potash taken into the stomach, and found to exist in the urine, could also be detected in other secretions, I find, on referring to my memorandums, the following particulars which I shall transcribe verbatim.

" August 19, 1807. Having heard from Dr. WOLLASTON, that prussiat of potash could be taken into the stomach with perfect safety, and that its presence could afterwards be discovered in the urine, but not in the serum; and being invited by him to follow up this inquiry, with a view to connect it with the theory of diabetes, I tried the following experiments.

Experiment 1.

" After having satisfied myself, by trials made by some medical gentlemen upon themselves, that considerable doses of prussiat of potash might be taken without the least inconvenience, I gave to a young woman labouring under diabetes mellitus, five grains of prussiat of potash dissolved in water, and this was repeated every hour till she had taken thirteen or fourteen such doses. After the fifth dose, her urine, by the addition of a drop or two of a solution of sulphat of iron, turned blue instantly. At this period of the experiment, a blister was applied to her stomach, and after a few hours, whilst still taking the prussiat of potash, and whilst the urine strongly indicated its presence, the blister was cut and the serum collected. This serous fluid being, in the same manner as the urine, subjected to the action of a solution of sulphat of iron, did not suffer any change of colour in the least indicative of the presence of prussic acid. Yet the urine still remained capable of imparting a blue colour to solution of iron, fifteen hours after taking the last dose of the prussiat of potash.

Experiment 2.

" The same person being soon afterwards put upon a course of ferruginous medicines, and having taken considerable quantities of sulphat of iron, an idea naturally occurred to me that the phenomenon might perhaps be reversed; but upon adding prussiat of potash to the urine, no vestige of iron could be discovered, and the same attempt was repeated several times with the same negative result.

Experiment 3.

" Dec. 2, 1807. The fluid obtained by means of a blister (as in Experiment 1,) being not immediately derived from the circulation, since it may be considered as the product of a secretion, I was desirous of repeating Dr. WOLLASTON'S experiment on the serum itself, under circumstances of impregnation similar to those in which the serum of the blister was examined.

" For this purpose, a young woman after taking, in divided doses, about a dram of prussiat of potash in the course of twelve hours, lost some blood by cupping, an operation which had been ordered for a local complaint under which she laboured. The serum having been allowed to separate, and a little nitric acid having been added to it, not the least vestige of prussic acid appeared in applying the test of sulphat of iron, although the urine made during the six hours which preceded and followed the cupping, was strongly impregnated with that acid, and struck a vivid blue upon adding the smallest quantity of iron."

I have only to observe, in addition to these particulars, that the susceptibility by which prussiat of potash is transmitted to the bladder, seems to vary in different individuals; for in five trials, made at Guy's Hospital in Nov. 1805, I failed of discovering any vestige of that salt in the urine of persons who had taken it in quantities sufficient to produce its appearance in others. Three of these individuals, I should observe, were at the time under mercurial treatment, and an idea occurred to me that mercury having a great affinity for prussic acid, the presence of that metal in the system might prevent the effect in question. But as in the two other failures, no mercury was present, I cannot lay any stress upon that conjecture. It may be proper to mention, that in the frequent trials which I have made with the prussiat of potash, no symptom or inconvenience whatever has ever occurred which could be ascribed to that salt.

I remain ever,
my dear Sir,
with great esteem, your's sincerely,
ALEX. MARCET.

P.S. Whilst revising the proof of this sheet, it has been observed to me by some friends, and in particular by Dr. HENRY of Manchester, and Dr. R. PEARSON of London, that in order to show distinctly that certain substances find their way to the bladder, without passing through the general circulation, it would be necessary to examine the arterial, as well as the venous blood, since it is not impossible that the whole of the sugar in diabetes, or the prussiat of potash in the experiments above related, may be conveyed to the urinary organs by the arteries, without entering the venous system. According to this hypothesis, it may be conceived that the same substances when conveyed by the arteries to distant parts of the body, may return by the absorbent system, and might in that case be discovered in the thoracic duct. This view of the subject may deserve further investigation; and I hope that this curious question will soon be decided by appropriate experiments.

VI. *On the Rectification of the Hyperbola by Means of Two Ellipses; proving that Method to be circuitous, and such as requires much more Calculation than is requisite by an appropriate Theorem: in which Process a new Theorem for the Rectification of that Curve is discovered.*

To which are added some further Observations on the Rectification of the Hyperbola: among which the great Advantage of descending Series over ascending Series, in many Cases, is clearly shown; and several Methods are given for computing the constant Quantity by which those Series differ from each other. By the Rev. John Hellins, *B. D. F. R. S. and Vicar of Potter's-Pury, in Northamptonshire.*

Being an Appendix to his former Paper on the Rectification of the Hyperbola, inserted in the Philosophical Transactions for the Year 1802. *Communicated by* Nevil Maskelyne, *D.D. F.R.S. Astronomer Royal.*

Read January 10, 1811.

1. THE rectification of the hyperbola by means of two ellipses is one proof, among many others, of the great sagacity of the late Mr. JOHN LANDEN, F. R.S.; and the ingenuity which he displayed on that occasion has obtained the notice and called forth the praises of eminent mathematicians both in this island and on the Continent. Yet, while the great ingenuity of the device is thus generally and justly allowed, this method of rectifying the hyperbola has always appeared to me to be

more curious than useful, as *it is circuitous, and requires much more calculation than is requisite for that purpose by an appropriate theorem.* The establishment of this truth is the main design of this short paper; a truth which, to my surprise, has not been noticed in any book that has come to my hands.*

2. But, before I proceed to investigation, it seems proper to remark, that Mr. LANDEN has, in his *Memoir*† on the Hyperbola and Ellipsis, expressed himself as if he thought that the difference between an hyperbolic arch and its tangent, when both are of an immense length, could not be computed before he published that work. His words at the beginning of the memoir above-mentioned, are these: "Some of the theorems "given by mathematicians for the calculation of fluents by "means of elliptic and hyperbolic arcs requiring, in the application thereof, the difference to be taken between an arc of "an hyperbola and its tangent; and such difference being "not directly attainable when such arc and its tangent both "become infinite, as they will do when the *whole* fluent is "wanted, although such fluent be at the same time finite; "those theorems therefore in that case fail, a computation "thereby being then impracticable without some farther "help."

"The supplying that defect I considered as a point of some "importance in geometry, and therefore I earnestly wished "and endeavoured to accomplish that business; my aim being "to ascertain, by means of such arcs as above-mentioned, the "*limit* of the difference between the hyperbolic arc and its

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* On this occasion, no one has betrayed more ignorance, nor shown a greater want of candour, than the writer of Art. XIII. in the Monthly Review for April 1803.

† This is the second in the 1st Volume of his *Memoirs*, printed in the year 1780.

" tangent, whilst the point of contact is supposed to be carried to an infinite distance from the vertex of the curve, " seeing that, by the help of that *limit*, the computation would " be rendered practicable in the case wherein, without such " help, the before-mentioned theorems fail. The result of " my endeavours respecting that point appears in this Memoir: which, among other matters, contains the investigation of a general theorem for finding the length of any arc " of any conic hyperbola by means of two elliptic arcs."—Vol. I. p. 23 and 24.

And towards the end of the same memoir he has expressed himself thus: " Mr. MACLAURIN's method of construction," [of the elastic curve,] " just now adverted to, though very " elegant, is not without a defect. The difference between the " hyperbolic arc and its tangent being necessary to be taken, " the method (for the reason mentioned at the beginning " of this Memoir) always fails when some principal point in " the figure is to be determined; the said arc and its tangent " then both becoming infinite, though their difference be at " the same time finite." P. 36.

3. Whoever reads the passages here quoted, and knows not what was done on the subject before Mr. LANDEN handled it, will undoubtedly conceive that he was the first person who solved the problem *of computing the difference between the length of the infinite arch of an hyperbola and its asymptote*. Yet the fact is not so. That difference may be computed, in many cases, by the first series given by Mr. MACLAURIN in Art. 808 of his *Treatise of Fluxions*, which series admits of an easy transformation into another form, by which the aforesaid difference may be computed in all cases; or the fluent may be

taken in a series which will always converge; and both he and Mr. Simpson have actually produced such a series, the one in the place before referred to, and the other in Art. 435 of his *Doctrine of Fluxions.* And although this series, when the transverse axis of the hyperbola is much greater than the conjugate, will converge very slowly, yet (as I have shown in the Philosophical Transactions for the year 1798,) the value of it to seven, or even to ten places of figures is, in all cases, attainable.

As Mr. Landen had the character of a man of great probity, and as he has, in various parts of his writings, shewn a regard for the memory of the eminent mathematicians above-mentioned, I cannot account for this misrepresentation of the matter any other way than by supposing that, being old when he wrote this memoir, and incumbered with much other business, his memory failed him. His just praise on this occasion is, that his solution of the aforesaid problem is much better than those of his contemporaries. I shall have occasion to speak again of this problem in my observations towards the end of this paper; but now proceed to the proof of the main point which I had in view in writing it.

4. If the transverse axis of any conic hyperbola be called 2, and the conjugate axis $2b$; and if the abscissa counted from the center on the transverse axis be called x, the corresponding ordinate y, and the length of the arch from the vertex to the ordinate H; then, by Simpson's *Fluxions*, Art. 435, we have $\dot{H} = \frac{\dot{x}\sqrt{(xx + bbxx - 1)}}{\sqrt{(xx - 1)}}$, or, (putting $ee = 1 + bb$,) $\dot{H} = \frac{\dot{x}\sqrt{(eexx - 1)}}{\sqrt{(xx - 1)}}$. [1]

5. If now we put $x = \sqrt{\left(\frac{1-\frac{uu}{ee}}{1-uu}\right)}$, [2] we shall have $xx = \frac{1-\frac{uu}{ee}}{1-uu}$, $eexx = \frac{ee-uu}{1-uu}$, and $eexx - 1 = \frac{ee-uu}{1-uu} - 1 = \frac{ee-uu-1+uu}{1-uu} = \frac{ee-1}{1-uu}$; we shall also have $xx - 1 = \frac{1-\frac{uu}{ee}}{1-uu} - 1 = \frac{1-\frac{uu}{ee}-1+uu}{1-uu} = \frac{\left(1-\frac{1}{ee}\right)uu}{1-uu}$; and thence, by substitution, $\frac{ee\,xx-1}{xx-1} = \frac{ee-1}{1-uu} \div \frac{\left(1-\frac{1}{ee}\right)uu}{1-uu} = \frac{e^4-ee}{(ee-1)uu} = \frac{ee}{uu}$, and consequently $\sqrt{\left(\frac{ee\,xx-1}{xx-1}\right)} = \frac{e}{u}$. [3]

But, since x was put $= \sqrt{\left(\frac{1-\frac{uu}{ee}}{1-uu}\right)}$, (see the equation numbered [2], we have $\dot{x} = \frac{-\frac{\dot{u}u}{ee}}{\sqrt{\left(1-\frac{uu}{ee}\right)}} \times \frac{1}{\sqrt{(1-uu)}} + \frac{\dot{u}u\sqrt{\left(1-\frac{uu}{ee}\right)}}{(1-uu)^{\frac{3}{2}}}$, which, by reduction, becomes $\frac{\left(1-\frac{1}{ee}\right)\dot{u}u}{(1-uu)^{\frac{3}{2}} \times \sqrt{\left(1-\frac{uu}{ee}\right)}}$. And lastly, by substituting this value of $\dot{x}$ in the equation numbered [3], we have $\dot{H} = \dot{x}\sqrt{\left(\frac{ee\,xx-1}{xx-1}\right)} = \frac{\left(1-\frac{1}{ee}\right)\dot{u}u}{(1-uu)^{\frac{3}{2}} \times \sqrt{\left(1-\frac{uu}{ee}\right)}} \times \frac{e}{u} = \frac{e\left(1-\frac{1}{ee}\right)\dot{u}}{(1-uu)^{\frac{3}{2}} \times \sqrt{\Big)1-\frac{uu}{ee}\Big)}}$,* the fluxion of the arch of the hyperbola.

6. Let us now (to simplify the expression) put $\varepsilon = \frac{1}{e}$, and

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* As this result differs from that given by Mr. WOODHOUSE, in p. 260 of the Philosophical Transactions for 1804, I have set down the process at large, that the intelligent reader may the more easily perceive where the truth lies.

assume $V = u\sqrt{\left(\frac{1-\varepsilon\varepsilon uu}{1-uu}\right)}$; then, by taking the fluxions on both sides, we shall have $\dot{V} = \dot{u}\sqrt{\left(\frac{1-\varepsilon\varepsilon uu}{1-uu}\right)} - \frac{\varepsilon\varepsilon \dot{u} uu}{\sqrt{(1-uu)}\times\sqrt{(1-\varepsilon\varepsilon uu)}} + \frac{\dot{u} uu \sqrt{(1-\varepsilon\varepsilon uu)}}{(1-uu)^{\frac{3}{2}}}$. Here $\dot{u}\sqrt{\left(\frac{1-\varepsilon\varepsilon uu}{1-uu}\right)}$, the first term on the right hand side of the equation, is evidently the fluxion of an arch of an ellipsis of which the transverse semi-axis is 1, the eccentricity is ε, and the abscissa (counted from the center) is u. The third term, $\frac{\dot{u} uu \sqrt{(1-\varepsilon\varepsilon uu)}}{(1-uu)^{\frac{3}{2}}}$, by multiplying both numerator and denominator by $\sqrt{(1-\varepsilon\varepsilon uu)}$, becomes $\frac{\dot{u} uu - \varepsilon\varepsilon \dot{u} u^4}{(1-uu)^{\frac{3}{2}}\times\sqrt{(1-\varepsilon\varepsilon uu)}}$, which (by division) is very easily resolved into $\frac{\varepsilon\varepsilon \dot{u} uu}{\sqrt{(1-uu)}\times\sqrt{(1-\varepsilon\varepsilon uu)}} + \frac{(1-\varepsilon\varepsilon)\dot{u} uu}{(1-uu)^{\frac{3}{2}}\times\sqrt{(1-\varepsilon\varepsilon uu)}}$; so that the sum of the second and third terms on that side of the equation becomes barely $\frac{(1-\varepsilon\varepsilon)\dot{u} uu}{(1-uu)^{\frac{3}{2}}\times\sqrt{(1-\varepsilon\varepsilon uu)}}$, which expression is obviously $= -\frac{(1-\varepsilon\varepsilon)\dot{u}}{\sqrt{(1-uu)}\times\sqrt{(1-\varepsilon\varepsilon uu)}} + \frac{(1-\varepsilon\varepsilon)\dot{u}}{(1-uu)^{\frac{3}{2}}\times(1-\varepsilon\varepsilon uu)}$; and the last of these (ε being by the notation $= \frac{1}{e}$) differs from $\dot{H}$, the fluxion of the hyperbolic arch, found in the preceding Article, only in that it is not multiplied by e; or, in other words, it is $= \frac{\dot{H}}{e}$. Let us therefore substitute the values now found for their equals in the above fluxional equation, and we shall have $\dot{V} = \dot{u}\sqrt{\left(\frac{1-\varepsilon\varepsilon uu}{1-uu}\right)} - \frac{(1-\varepsilon\varepsilon)\dot{u}}{\sqrt{(1-uu)}\times\sqrt{(1-\varepsilon\varepsilon uu)}} + \frac{\dot{H}}{e}$. (α)

7. Now it is easy to perceive, from what is done in Art. 13 and 14 of LANDEN's second *Memoir*, (above referred to,) that the fluent of the second term on the right-hand side of this equation may be found by means of the fluent of the first term,

an algebraic quantity, and the arch of another ellipsis which is more eccentric. And as this has been done by some late writers on the rectification of the ellipsis,* I shall, on the present occasion, only state and use the result, in a notation convenient for an arithmetical calculator. Thus, putting $\zeta = \frac{2\sqrt{e}}{1+e}$, and $vv = \frac{e+uu-\sqrt{(ee-uu)(1-uu)}}{2e}$,

and the fluent of $\frac{\dot{u}\sqrt{(1-\varepsilon\varepsilon uu)}}{\sqrt{(1-uu)}} = \text{E}$,

- - - of $\frac{\dot{u}}{\sqrt{(1-uu)} \times \sqrt{(1-\varepsilon\varepsilon uu)}} = \text{F}$,

- - - of $\frac{\dot{v}\sqrt{(1-\zeta\zeta vv)}}{\sqrt{(1-vv}} = {}^{`}\text{E}$, it is certain that

$$\text{F is} = \frac{\zeta\zeta}{2}\left(\frac{1+\varepsilon}{1-\varepsilon}\right)u + \frac{2}{1-\varepsilon\varepsilon}\text{E} - \frac{2}{1-\varepsilon}{}^{`}\text{E}.$$

Multiply this equation by $1-\varepsilon\varepsilon$, and write 2ε for its equal $\frac{\zeta\zeta}{2}(1+\varepsilon)^2$, and it becomes

$$(1-\varepsilon\varepsilon)\,\text{F} = 2\varepsilon u + 2\text{E} - 2(1+\varepsilon)\,{}^{`}\text{E}.$$

If we now take the fluents of the fluxions in the equation marked (α) in the preceding Article, we shall have $\text{V} = \text{E} - (1-\varepsilon\varepsilon)\,\text{F} + \frac{\text{H}}{e}$. And since all these quantities begin and increase together, this equation needs no correction. And by writing for $(1-\varepsilon\varepsilon)$ F its value found in the preceding equation, we have $\text{V} = -2\varepsilon u - \text{E} + 2(1+\varepsilon)\,{}^{`}\text{E} + \frac{\text{H}}{e}$; and thence by reduction and transposition, $\text{H} = 2\varepsilon e u + e\text{V} + e\text{E} - 2e(1+\varepsilon)\text{E}'$; which expression will be found to exceed that given (for the same purpose) by Mr. WOODHOUSE, in p. 261 of the Philosophical Transactions for the year 1804, in the ratio of e to 1.

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* See M. LACROIX's *Traité du Calcul Différentiel et du Calcul Intégral,* Tome II. p. 181.

Moreover; since by the notation in Art. 5 and 6, εe is $=1$, and $V = ux$, we have, by substituting these values for their equals in the last equation, $H = 2u + e(ux + E) - 2(1+e)$ $'E$, (ϛ) which is Landen's theorem in a different notation.

8. I am now to prove, that all the labour of computing the eccentricity* and abscissa, and the arch itself, of this second ellipsis, and the subsequent operations of multiplication and subtraction requisite in the application of it to the rectification of the hyperbola, is altogether needless; since the same end may be obtained by only computing and applying the fluent of $\dot{u}\sqrt{\left(\frac{1-uu}{1-\varepsilon\varepsilon uu}\right)}$, which will require no more calculation than must be made to find and apply the elliptic arch denoted by E (of which the axis, and eccentricity, and abscissa, are given). And the truth of this will quickly appear. For,

9. The fractional expression, $\frac{\dot{u}\sqrt{(1-\varepsilon\varepsilon uu)}}{\sqrt{(1-uu)}} - \frac{(1-\varepsilon\varepsilon)\dot{u}}{\sqrt{(1-uu)}\sqrt{(1-\varepsilon\varepsilon uu)}}$, found above, in the equation marked (α), (see Art. 6.) by reduction to a common denominator, becomes

$$\frac{\dot{u} - \varepsilon\varepsilon uu\dot{u} - \dot{u} + \varepsilon\varepsilon\dot{u}}{\sqrt{(1-uu)} \times \sqrt{(1-\varepsilon\varepsilon uu)}} = \frac{\varepsilon\varepsilon\dot{u}(1-uu)}{\sqrt{(1-uu)} \times \sqrt{(1-\varepsilon\varepsilon uu)}} = \frac{\varepsilon\varepsilon\dot{u}\sqrt{(-uu)}}{\sqrt{(1-\varepsilon\varepsilon uu}}.$$

Substitute this for its equal in the aforesaid equation, and we have $\dot{V} = \frac{\varepsilon\varepsilon\dot{u}\sqrt{(1-uu)}}{\sqrt{(1-\varepsilon\varepsilon uu)}} + \frac{\dot{H}}{e}$. ($\gamma$). Take the fluents, denoting that of $\frac{\dot{u}\sqrt{(1-uu)}}{\sqrt{(1-\varepsilon\varepsilon uu)}}$ by G, and there will be $V = \varepsilon\varepsilon G + \frac{H}{e}$; and thence, by transposition and multiplication, $H = eV - \varepsilon G$. (δ). This theorem, as far as I know, is new.

Here then it appears, that the rectification of the hyperbola is accomplished by means of the algebraic quantity eV,

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* When it is more convenient to use the conjugate semi-axis than the eccentricity, in the arithmetical calculation, then that must be computed instead of the eccentricity.

$(= eu\sqrt{\left(\frac{1-\varepsilon\varepsilon uu}{1-uu}\right)} = eux,)$ and $e \times$ the fluent of $\frac{\dot{u}\sqrt{(1-uu)}}{\sqrt{(1-\varepsilon\varepsilon uu)}}$. And it is obvious to every competent judge that the arithmetical work of computing the value of G, with any given values of ε and u, will be as short and easy as the computation of the elliptic arch denoted by E. Yet, for the more ready comparison of the series, with each other, which arise in taking these fluents, I will here set them down in the NEWTONIAN form, which undoubtedly is the most convenient for arithmetical calculations that has yet been discovered.

10. $\dot{\text{E}}$ is $= \frac{\dot{u}\sqrt{(1-\varepsilon\varepsilon uu)}}{\sqrt{(1-uu)}} = \frac{\dot{u}}{\sqrt{(1-uu)}} \times : 1 - \frac{\varepsilon\varepsilon uu}{2} - \frac{\varepsilon^4 u^4}{2.4} - \frac{3\varepsilon^6 u^6}{2.4.6} - \frac{3.5\varepsilon^8 u^8}{2.4.6.8}$, &c.; and denoting the fluents of $\frac{\dot{u}}{\sqrt{(1-uu)}}$, $\frac{\dot{u}\,uu}{\sqrt{(1-uu)}}$, $\frac{\dot{u}\,u^4}{\sqrt{(1-uu)}}$, &c. by A′, B′, C′, &c. respectively, we have

A′ = the circ. arch of which the rad. is 1, and sine u.

$\text{B}' = \frac{\text{A}' - u\sqrt{(1-uu)}}{2}$,

$\text{C}' = \frac{3\text{B}' - u^3\sqrt{(1-uu)}}{4}$,

$\text{D}' = \frac{5\text{C}' - u^5\sqrt{(1-uu)}}{6}$,

$\text{E}' = \frac{7\text{D}' - u^7\sqrt{(1-uu)}}{8}$,

&c. &c. And then, multiplying these quantities by their proper factors, and placing them in due order, we have

$$\text{E} = \text{A}' - \frac{\varepsilon\varepsilon}{2}\text{B}' - \frac{\varepsilon^4}{2.4}\text{C}' - \frac{3\varepsilon^6}{2.4.6}\text{D}' - \frac{3.5\varepsilon^8}{2.4.6.8}\text{E}', \text{ \&c.}$$

11. $\dot{\text{G}}$ is $= \frac{\dot{u}\sqrt{(1-uu)}}{\sqrt{(1-\varepsilon\varepsilon uu}} = \dot{u}\sqrt{(1-uu)} \times : 1 + \frac{\varepsilon\varepsilon uu}{2} + \frac{3\varepsilon^4 u^4}{2.4} + \frac{3.5\varepsilon^6 u^6}{2.4.6} + \frac{3.5.7\varepsilon^8 u^8}{2.4.6.8}$, &c. Now, denoting the fluents of $\dot{u}\sqrt{(1-uu)}$, $\dot{u}\,uu\sqrt{(1-uu)}$, $\dot{u}\,u^4\sqrt{(1-uu)}$, &c. by A, B, C, &c. respectively, we have

A = area of $\frac{1}{2}$ the mid. zone of a circ. of which the rad. is 1, and sine u.

$$B = \frac{A - u(1-uu)^{\frac{3}{2}}}{4},$$

$$C = \frac{3B - u^3(1-uu)^{\frac{3}{2}}}{6},$$

$$D = \frac{5C - u^5(1-uu)^{\frac{3}{2}}}{8},$$

$$E = \frac{7D - u^7(1-uu)^{\frac{3}{2}}}{10},$$

&c. &c. And, multiplying these quantities by their proper factors, and placing them in due order, we have

$$G = A + \frac{\varepsilon\varepsilon}{2} B + \frac{3\varepsilon^4}{2\,4} C + \frac{3.5\varepsilon^6}{2.4.6} D + \frac{3.5.7\varepsilon^8}{2.4.6.8} E, \&c.$$

12. Now, by comparing together the fluents denoted by A′ and A, B′ and B, C′ and C, &c. it is obvious that the arithmetical calculation of the one will not, in any respect, be more difficult than that of the other; and that A is always less than A′, B than B′, C than C′, &c. And it is evident that each of the series denoted by E and G converges by the same geometrical progression, viz. ε^2, ε^4, ε^6, &c. So that the arithmetical value of any number of terms of the latter series will always be nearer to the value of the whole, than the arithmetical value of the same number of terms of the former series will be to its whole. And as to transformations of the expression $\dot{u}\sqrt{\left(\frac{1-\varepsilon\varepsilon yu}{1-uu}\right)}$ into others, in order to obtain the fluent in series of swifter convergency, when the case requires it; it is obvious that similar operations may be performed on the expression $\dot{u}\sqrt{\left(\frac{1-uu}{1-\varepsilon\varepsilon uu}\right)}$.

In the application of E and G to the rectification of the hyperbola, the one is multiplied by e, the other by ε, (see the equations marked (β) and (δ) in Art. 7 and 9,) which are operations equally easy.

Thus it appears, that all the labour of computing the eccentricity, the abscissa, and the length of the elliptic arch denoted by ʽE, and of applying it to the rectification of the hyperbola, is wholly unnecessary; and consequently, that *that method is circuitous, and more curious than useful.*[*]

Some further Observations on the Rectification of the Hyperbola: among which the great Advantage of descending Series over ascending Series, in many cases, is clearly shewn; and several Methods are given for computing the constant Quantity by which those Series differ from each other.

13. The new series above given in Art. 11, it is obvious, will converge swiftly so long as u is small in comparison of 1; (which it will be when x is not much greater than 1:) so that this series will be very convenient for computing a small arch of an hyperbola near the vertex, even when ε is nearly $= 1$: and, when ε is small in comparison of 1, any arch, how great so ever, may easily be computed by it. But, when ε is nearly $= 1$, and uu is greater than $\frac{1}{2}$, this series will converge but slowly; and, for that reason, will not be an eligible form for arithmetical calculation. In such cases, however, a swift convergency will take place in some of the descending series (discovered by me, and) inserted in the Philosophical Trans-

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[*] By comparing the expressions marked γ and δ, in Article 9 of this Paper, with the short paragraph near the top of page 261 of the Philosophical Transactions for 1804, the mathematical reader will quickly perceive that Mr. WOODHOUSE has there asserted too much.

actions for 1802. And as those descending series differ from the ascending ones by a constant quantity, (as is there shown,) I will now add something to what was then said on the methods of computing the value of that constant quantity.

14. It is very evident from what is done in the Philosophical Transactions for 1802, from page 460 to page 465, that the constant quantity here spoken of is no other than the difference between the length of the arch of the hyperbola and its tangent, " when the point of contact" (to use LANDEN's words,) " is supposed to be carried to an infinite distance from " the vertex of the curve:" *—which difference is undoubtedly the same as " the difference between the length of the arch " infinitely produced and its asymptote"—(as SIMPSON † expresses it.) And since each of these eminent mathematicians, and MAC LAURIN also, (as I before observed,) has treated of this difference, it seems requisite that I should here give a brief statement of their methods of computing it, and compare them with such of my own as I offer to the public.

15. If 1 be written instead of a, in Art. 435 of SIMPSON's Fluxions, the eccentricity will be $\sqrt{1+bb}$, which is denoted by e in this Paper; and his $dd = \frac{1}{1+bb}$ will be $= \frac{1}{ee} = \varepsilon\varepsilon$. Substituting, therefore, 1 for a, and ε for d, in the series by which he expresses the difference between the length of the asymptote and the infinite arch, it becomes

$$A \times : \frac{\varepsilon}{2} + \frac{\varepsilon^3}{2.2.4} + \frac{3.3\varepsilon^5}{2.2.4\,4.6} + \frac{3.3.5.5\varepsilon^7}{2.2.4.4.6.6.8}, \text{ \&c.}$$

his A being = the quadrantal arch of a circle of which the radius is 1.

In like manner, 1 being written instead of a, in the second

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[*] See LANDEN's Memoirs, Vol. I. p. 23.

[†] See his Fluxions, Art. 435.

series in Art. 808 of MAC LAURIN's Fluxions, his $E = a + \frac{bb}{a}$ will become $= 1 + bb$, which is $= ee$ in the notation used in this paper; and his series,

$$\frac{Na}{2} \times \sqrt{\frac{a}{E}} + \frac{aA}{2.4E} + \frac{9aB}{4.6E} + \frac{25aC}{6.8E} + \frac{49aD}{8.10E}, \text{ \&c.}$$

(A denoting the first term, B the second, C the third, &c.) will become $N \times : \frac{1}{2e} + \frac{1}{2.2.4e^3} + \frac{3.3}{2.2.4.4\ 6e^5} + \frac{3.3.5\ 5}{2.2.4.4.6.6\ 8e^7}$, &c. which series, since $\frac{1}{e}$ is $= \varepsilon$, and N denotes the quadrantal arch of a circle of which the radius is 1, exactly agrees with SIMPSON's series.

And this series will be found to agree also with the value of εG in the equation marked (δ) in Art. 9, when u becomes $= 1$. For, in that case, $eV = ex$ denotes the asymptote, and H the infinite arch of the hyperbola; and we have, by transposition, $ex - H = \varepsilon G$. And, u being $= 1$, A, the first term of the series denoted by G in Art. 11, becomes = the area of a quadrant of a circle of which the radius is 1, that is, $= \frac{N}{2}$: B becomes $= \frac{A}{4} = \frac{N}{2.4}$: C becomes $= \frac{3B}{6} = \frac{3N}{2.4\ 6}$: D becomes $= \frac{5C}{8} = \frac{3.5N}{2.4.6.8}$: &c. and these values being written for A, B, C, D, &c. and the whole $\times\ \varepsilon$, we have $\varepsilon G = N \times : \frac{\varepsilon}{2} + \frac{\varepsilon^3}{2.2.4} + \frac{3.3\varepsilon^5}{2.2.4.4.6} + \frac{3.3.5.5\varepsilon^7}{2.2.4.4.6.6.8}$, &c. perfectly agreeing with the series above stated.

This series, it is obvious, will converge but slowly when e is not much greater, and consequently ε not much less, than 1; that is when b is small in comparison of 1. But, in such cases, other series which have a good rate of convergency may be used, as was shown in my former paper on the Rectification of the Hyperbola, and will more fully appear in the following pages.

Mr. Landen's methods of computing the aforesaid difference next come under consideration: and, first, his method of computing it by means of the arches of two ellipses.

16. We have seen above, that, when x becomes immensely great, u becomes $= 1$; and in this case the equation ($\mathfrak{C}$) in Art. 7 becomes $H = 2 + ex + eE - 2\,(1 + e)\,{}^{\backprime}E$; from which we have $ex - H = 2\,(1 + e)\,{}^{\backprime}E - eE - 2$, another expression of the difference between the length of the asymptote and the infinite arch: which expression, however, is not so convenient for arithmetical calculation as the preceding. For here E denotes the quadrantal arch of an ellipsis of which the transverse semi-axis is 1, and the eccentricity ε; which arch is $=$

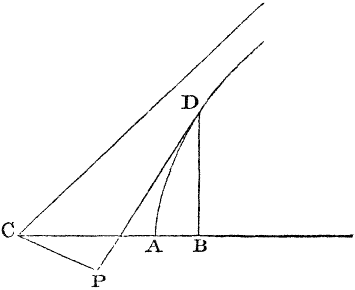
$$N \times : 1 - \frac{\varepsilon\varepsilon}{2.2} - \frac{3\varepsilon^4}{2.2.4.4} - \frac{3\cdot3\cdot5\varepsilon^6}{2.2.4.4.6.6} - \frac{3\cdot3\cdot5\cdot5\cdot7\varepsilon^8}{2.2.4.4.6.6.8.8}, \&c.$$

so that the computation of $eE = \frac{E}{\varepsilon} =$

$$N \times : e - \frac{\varepsilon}{2.2} - \frac{3\varepsilon^3}{2.2.4.4} - \frac{3\cdot3\cdot5\varepsilon^5}{2.2.4.4.6.6} - \frac{3\cdot3\cdot5\cdot5\cdot7\varepsilon^7}{2.2.4.4.6.6.8.8}, \&c.$$

will require as much labour as the computation of εG, which is the very difference sought. (See the preceding Art.) But, by this method, we have yet to compute the elliptic arch denoted by ${}^{\backprime}E$, of which the transverse semi-axis is 1, the eccentricity $\zeta = \frac{2\sqrt{\varepsilon}}{1+\varepsilon}, \left(= \frac{2\sqrt{e}}{1+e},\right)$ and abscissa $v = \sqrt{\left(\frac{\varepsilon+1}{2}\right)}, \left(= \sqrt{\left(\frac{e+1}{2e}\right)}\right)$; and then there must be a multiplication of this quantity by $2\,(1 + e)$, and, after that, a subtraction of $eE + 2$ from the product, to obtain the difference sought: all which labour is more than is required by the method described in the preceding article. Here, then, we have a striking instance (and a thousand more might be produced) of the inutility of rectifying the hyperbola by means of two ellipses.

17. But Mr. LANDEN discovered another and a better method of computing the difference in question, which is briefly this:



If from C, the center of an hyperbola, CP be drawn perpendicular to DP, a tangent to the curve in D; and if the transverse semi-axis CA be $= m$, the conjugate semi-axis $= n$, the perpendicular CP $= p$; and if f be put $= \frac{mm-nn}{2m}$, and $z = \frac{pp}{m}$; then will the difference between the length of the tangent DP and the arch AD be universally expressed by d — the fluent of $\frac{\dot{z}\sqrt{mz}}{2\sqrt{(nn+2fz-zz)}}$, where d is a constant quantity $=$ the fluent generated while z increases from o to m.

If now $\frac{pp}{m}$ be written instead of z, the above fluxion will become $\frac{\dot{p}pp}{\sqrt{(m^2n^2+2fm-p^4)}}$, agreeing with MAC LAURIN's expression of the same thing in Art. 804 and 808 of his Fluxions, where the transverse and conjugate semi-axes are denoted by a and b, respectively; and, in the latter of those articles, this expression is resolved into $\frac{\dot{p}pp}{\sqrt{(aa-pp)}\sqrt{(bb+pp)}}$, and its fluent generated while p increases from o to a, or m, is exhibted in series of which I have spoken in Art. 3 and 15 of this Paper.

Mr. LANDEN has no where, that I know of, exhibited the fluent of the above fluxion in series, but in the following manner.* Denoting the fluent of $\frac{\dot{p}pp}{\sqrt{(mm-pp)}\sqrt{(nn+pp)}}$, generated while p increases from o to m, by L, (which I denote by d,) he says that, when the abscissa CB is $= m \times \surd\left(1 + \frac{n}{\surd(mm+nn)}\right)$, (at which time the ordinate BD is $= n \times \surd\left(\frac{n}{\surd(mm+nn)}\right)$, and the tangent DP $= \surd(mm + nn)$), then L, (or d,) is $= 2$ DP $- 2$ AD $+ n - \surd(mm + nn) = n + \surd(mm + nn) - 2$ AD.

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* See his 2d Memoir, Art. 5.

18. Now, in order to compare this method with those of Mac Laurin and Simpson which have been described above, we may proceed thus: $\frac{\dot{p}pp}{\surd(mm-pp)\surd(nn+pp)}$ (which, for the sake of brevity in a subsequent use of the fluent, I denote by $\dot{\phi}$,) is $= \frac{\dot{p}pp}{m\surd(nn+pp)} \times : 1 + \frac{pp}{2mm} + \frac{3p^4}{2.4m^4} + \frac{3.5p^6}{2.4.6m^6} + \frac{3.5.7p^8}{2.4.6.8m^8}$, &c.; it is also

$$= \frac{\dot{p}pp}{n\surd(mm-pp)} \times : 1 - \frac{pp}{2nn} + \frac{3p^4}{2.4n^4} - \frac{3.5p^5}{2.4.6n^6} + \frac{3.5.7p^8}{2.4.6.8n^8}, \text{ \&c.};$$

the one series proceeding by the powers of $\frac{pp}{mm}$, the other by the powers of $\frac{pp}{nn}$; which geometrical progressions, assisted by numeral coefficients, it is obvious, will have place also in the fluents as they are exhibited here below. Thus,

The fluents of $\frac{\dot{p}pp}{\surd(nn+pp)}$, $\frac{\dot{p}p^4}{\surd(nn+pp)}$, $\frac{\dot{p}p^6}{\surd(nn+pp)}$, &c. being denoted by A, B, C, &c. respectively, we shall have

$$A = \frac{p\surd(nn+pp) - nn\,\text{H. L.}\left(\frac{p}{n} + \surd\left(1 + \frac{pp}{nn}\right)\right)}{2},$$

$$B = \frac{p^3\surd(nn+pp) - 3nn\,A}{4},$$

$$C = \frac{p^5\surd(nn+pp) - 5nn\,B}{6},$$

$$D = \frac{p^7\surd(nn+pp) - 7nn\,C}{8},$$

$$E = \frac{p^9\surd(nn+pp) - 9nn\,D}{10},$$

&c. &c.

and hence,

$$1^\circ.\ \varphi = \frac{1}{m} \times : \mathrm{A} + \frac{1}{2mm}\mathrm{B} + \frac{3}{2.4m^4}\mathrm{C} + \frac{3.5}{2.4.6m^6}\mathrm{D} + \frac{3.5.7}{2.4.6.8m^8}\mathrm{E}, \&c.$$

And the fluents of $\frac{\dot{p}pp}{\sqrt{(mm-pp)}}$, $\frac{\dot{p}p^4}{\sqrt{(mm-pp)}}$, $\frac{\dot{p}p^6}{\sqrt{(mm-pp)}}$, &c. being denoted by A′, B′, C′, &c. respectively, we shall have

$$\mathrm{A}' = \frac{mm \times \text{cir. arch of which rad. is 1 and sine } \frac{p}{m}, - p\sqrt{(mm-pp)}}{2},$$

$$\mathrm{B}' = \frac{3mm\mathrm{A}' - p^3\sqrt{(mm-pp)}}{4},$$

$$\mathrm{C}' = \frac{5mm\mathrm{B}' - p^5\sqrt{(mm-pp)}}{6},$$

$$\mathrm{D}' = \frac{7mm\mathrm{C}' - p^7\sqrt{(mm-pp)}}{8},$$

$$\mathrm{E}' = \frac{9mm\mathrm{D}' - p^9\sqrt{(mm-pp)}}{10},$$

&c. &c.

and hence,

$$2^\circ.\ \varphi = \frac{1}{n} \times : \mathrm{A}' - \frac{1}{2nn}\mathrm{B}' + \frac{3}{2.4n^4}\mathrm{C}' - \frac{3.5}{2.4.6n^6}\mathrm{D}' + \frac{3.5.7}{2.4.6.8n^8}, \mathrm{E}', \&c.$$

Each of these series begins and increases with p; so that neither of them needs any correction.

19. Two series being thus obtained as expressions of the general value of the fluent of $\frac{\dot{p}pp}{\sqrt{(mm-pp)}\sqrt{(nn+pp)}}$, (denoted by φ,) the next thing to be done is, to ascertain the rate of convergency of each when the abscissa CB is $= m\sqrt{\left(1 + \frac{n}{(mm+nn)}\right)}$. Now to this value of the abscissa CB, the corresponding value of the perpendicular CP $= p$ (as appears from the equations in Art. 17,) is $= m\sqrt{\left(\frac{n}{n+\sqrt{(mm+nn)}}\right)}$. We therefore have, in this case, $\frac{pp}{mm} = \frac{n}{n+\sqrt{(mm+nn)}}$, and $\frac{pp}{nn} = \frac{mm}{nn+n\sqrt{(mm+nn)}}$, which are the respective rates of convergency of the geometrical progressions which (assisted by numeral coefficients) will have place in the first and second series given in the preceding article.

20. It is now easy to compare these rates of convergency with that which has place in the series in Art. 15, thus: Putting 1 instead of m, and b instead of n, the rates are these, viz.

In 1st Series.	In 2d Series.	In Art. 15.
$\frac{b}{b+\surd(1+bb)}$,	$\frac{1}{bb+b\surd(1+bb)}$,	$\frac{1}{1+bb}$.

And writing 1, 2, 3, &c. successively instead of b, we have

$\frac{1}{1+\surd 2}$,	$\frac{1}{1+\surd 2}$,	$\frac{1}{2}$,
$\frac{2}{2+\surd 5}$,	$\frac{1}{4+2\surd 5}$,	$\frac{1}{5}$,
$\frac{3}{3+\surd 10}$,	$\frac{1}{9+3\surd 10}$,	$\frac{1}{10}$,
&c.	&c.	&c.

In all which cases it is evident, that the calculation by the series given in Art. 15 will, on a double account of simplicity, be much easier than by the second series in the preceding Art. notwithstanding its greater rate of convergency.

Let us now write $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, &c. successively instead of b, and we shall have the following rates

In 1st Series.	In 2d Series.	In Art. 15.
$\frac{1}{1+\surd 5}$,	$\frac{4}{1+\surd 5}$,	$\frac{4}{5}$,
$\frac{1}{1+\surd 10}$,	$\frac{9}{1\surd+10}$,	$\frac{9}{10}$,
$\frac{1}{1+\surd 17}$,	$\frac{16}{1+\surd 17}$,	$\frac{16}{17}$,
&c.	&c.	&c.

Here the great advantage of the first series given in the preceding Art. over that which was discovered by MAC LAURIN and SIMPSON appears; and we see that LANDEN had good grounds for valuing his method, or, to express myself better, one of his methods of solving the Problem which I have now under consideration, although it cannot be truly said that he finished his work.

21. If indeed the hyperbola were equilateral, then, n being $= m$, the fluxional expression $\frac{\dot{p}pp}{\surd(mm-pp)\surd(nn+pp)}$ becomes $=$ $\frac{\dot{p}pp}{\surd(m^4-p^4)} = \frac{\dot{p}pp}{mm} \times : 1 + \frac{p^4}{2m^4} + \frac{3p^8}{2.4m^8} + \frac{3.5p^{12}}{2.4.6m^{12}}$, &c.; and we have

$$\varphi = \frac{p^3}{mm} \times : \frac{1}{3} + \frac{p^4}{2.7m^4} + \frac{3p^8}{2.4.11m^8} + \frac{3.5p^{12}}{2.4.6.15m^{12}}, \text{ \&c.}$$

And taking $p = m\surd\left(\frac{n}{n+\surd(mm+nn)}\right)$, which, in this case, is $= m\surd\left(\frac{1}{1+\surd 2}\right)$, we shall have $\frac{p^4}{m^4} = \frac{m^4}{m^4}\left(\frac{1}{1+\surd 2}\right)^2 = \frac{1}{3+\surd 8}$, the rate of convergency of the geometrical progression which will have place in this series; and this rate of convergency, together with the simplicity of the numeral coefficients, will render this series eligible for numerical calculation in preference to either of the other series.

22. The fluent of $\frac{\dot{p}pp}{\surd(mm-pp)\surd(nn+pp)}$, when $p = m\surd\left(\frac{n}{n+\surd(mm+nn)}\right)$, being obtained in *converging series*, whatever be the ratio of m to n; let this particular value of it be denoted by Φ, (to distinguish it from the general value denoted by φ,) and substituted in the general equation $d - \varphi = \text{DP} - \text{AD}$, in Art. 17, and we shall have $d - \Phi = \text{DP} - \text{AD}$; and this value of DP − AD being substituted for it in the particular equation $\text{L} = d = 2\text{DP} - 2\text{AD} + n - \surd(mm + nn)$, in the same

Article (in which equation $\mathrm{DP} = \surd(mm + nn)$,) we have $d = 2d - 2\Phi + n - \surd(mm + nn)$, and hence $\mathrm{L} = d = 2\Phi + \surd(mm + nn) - n$, which is the difference, or quantity sought.

23. I come now to make a comparison of Mr. Landen's last mentioned method of computing the difference in question with some of my own methods.

We have already seen in Art. 20, that, when the conjugate axis of the hyperbola is greater than the transverse, Landen's method is not wanted, since the operation by the old series will, in general, be easier. The comparison therefore, now to be made, is only in cases when the conjugate axis is equal to, or less than, the transverse axis.

It appears from Art. 9, 11, and 17 of this Paper, that, when m is put $= 1$, and x is taken $= m\surd\left(1 + \frac{n}{\surd(mm+nn)}\right)$, the value of $\varepsilon\varepsilon uu$ is $\frac{1}{1+nn+n\surd(1+nn)}$, the powers of which fraction form the geometrical progression which will have place in the series denoted by G, from which H, or the arch AD is quickly obtained. And, with these values of m and x, we have, by Art. 19, $\frac{pp}{mm} = \frac{n}{n+\surd(1+nn)}$, the powers of which fraction form the geometrical progression which will have place in the series denoted by Φ. When n is taken $= 1$, the former of these algebraic fractions is $= \frac{1}{2+\surd 2}$, the latter is $= \frac{1}{1+\surd 2}$. As the one fraction increases with n, and the other decreases, it is easy to find that value of n which shall make them equal, viz. $n = \frac{1}{2}\surd(-2 + \surd 20) = 0{\cdot}786$, &c.; and, with this value of n, each of these fractions is $= \frac{3-\surd 5}{2} = 0{\cdot}381966$, &c. Thus it appears, that, n having any value greater than $\frac{1}{2}\surd(-2 +$

$\sqrt{}20$), the fraction $\frac{1}{1+nn+n\sqrt{(1+nn)}}$ will be less than $\frac{\cdot}{n+\sqrt{(1+nn)}}$, and less than 0·381966; and consequently that the hyperbolic arch AD may be more easily obtained from the series denoted by G, in Art. 11, than by either of those denoted by φ in Art. 18. And hence we may derive another expresssion of the value of *d*, in the following manner.

24. Since uu is universally $= \frac{ee\,xx-ee}{ee\,xx-1}$, (see equation [3] in Art. 5, where *e* denotes the eccentricity;) by putting $xx = 1 + \frac{n}{\sqrt{(1+nn)}}$, and writing $1+nn$ instead of ee, we shall have $uu = \frac{\sqrt{(1+nn)}}{n+\sqrt{(1+nn)}}$, and $\varepsilon\varepsilon\,uu = \frac{uu}{ee} = \frac{1}{1+nn+n\sqrt{(1+nn)}}$. Let the value of G, corresponding to these values of *u* and *e*, be denoted by Γ; then, by the equation (δ) in Art. 9, the hyperbolic arch AD $=$ H, is $= eV - \varepsilon\Gamma$. But, since V was put $= ux$, (see Art. 5 and 6,) it will in this case be $= 1$. Writing therefore $e - \varepsilon\Gamma$ for AD, and 1 for *m*, in the equation at the end of Art. 17, we have $d = n + \sqrt{}(1+nn) - 2e + 2\varepsilon\Gamma = 2\varepsilon\Gamma + n - \sqrt{}(1+nn)$. [$\eta$]

It now appears, that *d*, the difference between the length of an infinite arch of an hyperbola and its tangent, or asymptote, may be computed by means of one series converging swifter than the powers of $\frac{4}{10}$, even in the most disadvantageous case; so that a dozen terms of it will be sufficient for all common uses: but, that a series of such convergency was attainable in this case, appears not to have been observed by either of the writers before mentioned.

25. If the transverse and conjugate semi-axes of an hyperbola are denoted by *a* and 1, respectively, the ordinate by *y*, and the arch by *z*; and if $\sqrt{}(aa+1)$, the eccentricity, be

put $= e$, and if $\frac{1}{\sqrt{e}}$ be written instead of y, in Theorems II. and IV. in pages 453 and 454 of the Philos. Trans. for 1802; then, (as appears by pages 466 and 467 of the same Vol.) the corresponding value of z may be obtained from either of the following equations, viz.

1. $z = A - \frac{1}{2} B + \frac{3}{2.4} C - \frac{3.5}{2.4.6} D + \frac{3.5.7}{2.4.6.8} E$, &c.

2. $$z + d = \begin{cases} e\sqrt{(\frac{1}{e} + 1)} \\ + \frac{1}{2e} A' - \frac{1}{2.4.e^3} B' + \frac{3}{2.4.6e^5} C' - \frac{3.5}{2.4.6.8e^7} D', \text{\&c.} \end{cases}$$

in the first of which equations

$$A = \frac{1}{2\sqrt{e}} \sqrt{(1+e)} + \frac{1}{2e} \text{ H.L. } (\sqrt{e} + \sqrt{(1+e)}),$$

$$B = \frac{e^{-\frac{1}{2}}(1+e)^{\frac{3}{2}} - A}{4ee},$$

$$C = \frac{e^{-\frac{3}{2}}(1+e)^{\frac{3}{2}} - 3B}{6ee},$$

$$D = \frac{e^{-\frac{5}{2}}(1+e)^{\frac{3}{2}} - 5C}{8ee},$$

$$E = \frac{e^{-\frac{7}{2}}(1+e)^{\frac{3}{2}} - 7D}{10ee},$$

&c. &c.

in the second,

$$A' = \text{H.L. } (\sqrt{1} + e) - \sqrt{e}),$$

$$B' = \frac{-e\sqrt{(\frac{1}{e} + 1)} - A'}{2},$$

$$C' = \frac{-ee\sqrt{(\frac{1}{e} + 1)} - 3B'}{4},$$

$$D' = \frac{-e^3\sqrt{(\frac{1}{e} + 1)} - 5C'}{6},$$

$$E' = \frac{-e^4\sqrt{(\frac{1}{e} + 1)} - 7D'}{8},$$

&c. &c.

Now, since $\sqrt{(1+e)} - \sqrt{e}$ is $= \frac{1}{\sqrt{(1+e)}+\sqrt{e}}$, if l be put $=$ H. L. $(\sqrt{e}+\sqrt{(1+e)})$, and if the values of A, B, C, &c. and of A′, B′, C′, &c. be taken in terms of e and l, and written for them in the above two equations, we shall have

$$\begin{aligned}
1.\ z = {} & \sqrt{1+e} \times \frac{1}{2e^{\frac{1}{2}}} + \frac{l}{2e} \\
& - \sqrt{1+e} \times \left(\frac{1}{2.4e^{\frac{3}{2}}} + \frac{1}{2.2.4e^{\frac{5}{2}}}\right) + \frac{l}{2.2.4e^{3}} \\
& + \sqrt{1+e} \times \left(\frac{3}{2.4.6.e^{\frac{5}{2}}} + \frac{3}{2.4.4.6e^{\frac{7}{2}}} - \frac{3.3}{2.2.4.4.6e^{\frac{9}{2}}}\right) + \frac{3.3l}{2.2.4.4.6e^{5}} \\
& - \sqrt{1+e} \times \left(\frac{3.5}{2.4.6.8e^{\frac{7}{2}}} + \frac{3.5}{2.4.6.6.8e^{\frac{9}{2}}}, - \frac{3.5.5}{2.4.4.6.6.8e^{\frac{11}{2}}} + \frac{3.3.5.5}{2.2.4.4.6.6.8e^{\frac{13}{2}}}\right) + \frac{3.3.5.5l}{2.2.4.4.6.6.8e^{7}} \\
& \qquad\qquad \&c. \qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad \&c.
\end{aligned}$$

$$\begin{aligned}
2.\ z + d = {} & e^{\frac{1}{2}}\sqrt{1+e} - \frac{l}{2e} \\
& + \sqrt{1+e} \times \frac{1}{2.2.4e^{\frac{5}{2}}} - \frac{l}{2.2.4e^{3}} \\
& - \sqrt{1+e} \times \left(\frac{3}{2.4.4.6e^{\frac{7}{2}}} - \frac{3.3}{2.2.4.4.6e^{\frac{9}{2}}}\right) - \frac{3.3l}{2.2.4.4.6e^{5}} \\
& + \sqrt{1+e} \times \left(\frac{3.5}{2.4.6.6.8e^{\frac{9}{2}}} - \frac{3.5.5}{2.4.4.6.6.8e^{\frac{11}{2}}} + \frac{3.3.5.5}{2.2.4.4.6.6.8e^{\frac{13}{2}}}\right) - \frac{3.3.5.5l}{2.2.4.4.6.6.8e^{7}} \\
& \qquad\qquad \&c. \qquad\qquad\qquad\qquad\qquad\qquad\qquad \&c.
\end{aligned}$$

Here, on the right-hand side of both these equations, the diagonal lines of quantities in which l enters, and the perpendicular columns which have the common factor $\sqrt{1+e}$, the first column of the first equation, and the first term of the second excepted, are exactly alike, but under contrary signs; so that, by taking the sum on each side, we have

$$2z + d = \begin{cases} \sqrt{1+e} \times : \frac{1}{2e^{\frac{1}{2}}} - \frac{1}{2.4e^{\frac{3}{2}}} + \frac{3}{2.4.6e^{\frac{5}{2}}} - \frac{3.5}{2.4.6.8e^{\frac{7}{2}}} \ \&c. \\ + e^{\frac{1}{2}}\sqrt{1+e}. \end{cases}$$

The right-hand side of this equation, by multiplying both numerators and denominators by $e^{\frac{1}{2}}$, becomes $e^{\frac{1}{2}}\sqrt{1+e} \times : 1 + \frac{1}{2e} - \frac{1}{2.4ee} + \frac{3}{2.4.6e^3} - \frac{3.5}{2.4\,6.8e^4}$, &c. which is $= e^{\frac{1}{2}}\sqrt{1+e} \times \sqrt{1+\frac{1}{e}} = \sqrt{1+e} \times \sqrt{1+e} = 1+e$. Hence we have $d = 1 + e - 2z$, which is the very equation at the end of Art. 17, obtained by LANDEN's method, the difference being only in the notation.

It appears by this result, that the constant difference between the values of the ascending and descending series, here denoted by d, is equal to the difference between the lengths of the infinite arch and its tangent, as was observed in Art. 14, and may be briefly proved thus: by the notation specified at the beginning of this Art. and the property of the curve, as $1 : e :: y : ey$, the length of a line drawn parallel to the asymptote from the extremity of the ordinate to the transverse axis; which line, when y becomes immensely great, will coincide with the tangent drawn from the same point, and will be equal to the corresponding portion of the asymptote. And it appears by Art. 13 of my former Paper on the Rectification of the Hyperbola, (see Philos. Trans. for 1802, p. 461,) that the corresponding arch of the hyperbola, z, is $= ey - d$. We therefore have, in this case, $d =$ *the asymptote—the infinite arch.*

26. Thus is LANDEN's best theorem respecting the rectification of the hyperbola obtained by the common application of Sir ISAAC NEWTON's doctrine of infinite series. And I further observe, *in transitu*, that FAGNANI's theorem, respecting the rectification of the ellipsis, is attainable in the same easy manner. These devices are indeed very ingenious; but their

utility appears to me to be much less than has been imagined. It has been represented even in these Transactions, for the year 1804, p. 236, that FAGNANI's theorem is necessary to the investigation of EULER's series for computing the length of a quadrantal arch of an ellipsis; yet, the fact is, that FAGNANI's theorem is no more requisite on that occasion than LANDEN's theorem is in the investigation of a similar series for computing the difference between the lengths of an infinite arch of an hyperbola and its asymptote, which will be given in this Paper.

27. It appears by inspecting the values of z and $z + d$, exhibited in terms of e and l, in Art. 25, that when y becomes equal to, or greater than, $\frac{1}{\sqrt{e}}$, Theorem IV. will be more eligible for arithmetical calculation than Theorem II. It is obvious also, from the same article, that no more than one of those values need be computed in order to obtain the value of d. Putting, therefore, ζ for the value of z corresponding to $y = \frac{1}{\sqrt{e}}$, and

$$S = \begin{cases} \sqrt{e + ee} \\ + \frac{1}{2e} A' - \frac{1}{2.4e^3} B' + \frac{3}{2.4.6e^5} C', \text{ \&c.} \end{cases}$$

(A′, B′, C′, &c. being as there specified,) we have $\zeta = S - d$; and this value being written for ζ in the equation $d = 1 + e - 2\zeta$, it becomes $d = 1 + e + 2d - 2S$; from which we have $d = 2S - 1 - e$, which is another convenient *formula* for computing the value of d.

28. If the diagonal line of quantities in which l enters, either in the value of z or $z + d$, before referred to, were written by itself, and the remaining perpendicular columns summed in the manner by me described in the Philos. Trans. for 1798, p. 548 *et seq.* the value of d might be obtained in a pair of

series, each of them converging by the powers of $\frac{1}{ee}$. But the advantage of computing by such a pair of series, instead of the single one above described, is less than might at first be imagined; for, in order to have a result true to the same number of figures, about the same number of terms must be computed, whether of the single series or of the pair. Since, therefore, the advantage obtained by such a transformation lies not in the literal powers, it can have place only in the coefficients; and there it may be very considerable.

I now proceed to the investigation of a pair of series for computing the value of the constant quantity d, each of which converges by the powers of $\frac{1}{aa}$.

29. If the transverse and conjugate semi-axes of an hyperbola are denoted by a and 1, respectively, the ordinate by y, and the corresponding arch by z; and if the eccentricity, $\sqrt{aa+1}$, be denoted by e, and $1+eeyy$ be put $=uu$; then, as I have shewn in the Philos. Trans. for 1802, p. 457, will

$$z=\begin{cases}\sqrt{uu+aa}+\frac{1}{2}\,\mathrm{A}+\frac{3}{2.4}\,\mathrm{B}+\frac{3.5}{2.4.6}\,\mathrm{C}+\frac{3.5.7}{2.4.6.8}\,\mathrm{D}, \&c. \\ -d;\end{cases}$$

$$\begin{aligned} \text{where } \mathrm{A} &= \frac{1}{a}\,\mathrm{H.L.}\,\frac{\sqrt{(aa+uu)}-a}{u}, \\ \mathrm{B} &= \frac{-\sqrt{(uu+aa)}}{2aa\;uu}-\frac{\mathrm{A}}{2aa}, \\ \mathrm{C} &= \frac{-\sqrt{(uu+aa)}}{4aa\;u^4}-\frac{3\mathrm{B}}{4aa}, \\ \mathrm{D} &= \frac{-\sqrt{(uu+aa)}}{6aa\;u^6}-\frac{5\mathrm{C}}{6aa}, \\ &\&c. \qquad\qquad \&c. \end{aligned}$$

and d, the constant quantity to be subtracted from the series, is (see p. 462 of the Philos. Trans. for 1802, and Art. 25;)

equal to the difference between the lengths of the infinite arch and its asymptote.

Now, when $y = o$, then $z = o$, and u becomes $= 1$; and the value of the series, in this case, is the value of d; and this value, if the terms of the series are ranged one under another according to the powers of $\frac{1}{aa}$, will stand as below, viz. $d =$

$$\begin{array}{llll}
\sqrt{1+aa}\,\Big| + \frac{A}{2} & & & \\
-\frac{3\sqrt{(1+aa)}}{2.4.2aa}\Big| & -\frac{3A}{2.4.2aa} & & \\
-\frac{3\cdot5\sqrt{(1+aa)}}{2.4.6.4aa} & +\frac{3\cdot5\cdot3\sqrt{(1+aa)}}{2.4.6.4.2a^4}\Big| & +\frac{3\cdot5\cdot3A}{2\ 4\ 6.4.2a^4} & \\
-\frac{3\ 5\cdot7\sqrt{(1+aa)}}{2.4.6.8.6aa} & +\frac{3\cdot5\ 7\cdot5\sqrt{(1+aa)}}{2.4\ 6.8.6.4a^4} & -\frac{3\cdot5\cdot7\cdot5\cdot3\sqrt{(1+aa)}}{2.4.6.8.6.4.2a^6}\Big| & -\frac{3\cdot5\cdot7\cdot5\cdot3A}{2.4.6.8.6.4\ 2a^6} \\
\&c. & \&c. & \&c. & \&c.
\end{array}$$

Here, in the diagonal line of quantities which have the common factor A, we find the geometrical progression $\frac{1}{aa}$, $\frac{1}{a^4}$, $\frac{1}{a^6}$, &c. so that, when a is much greater than 1, this series will converge very swiftly. The same progression has place also in the perpendicular columns of quantities which remain after the diagonal line is taken away; in all which columns we find the constant factor $\sqrt{1+aa}$. If, therefore, the sums of the very slowly converging numeral series

$$\frac{3}{2.4.2} + \frac{3\cdot5}{2\ 4.6.4} + \frac{3\cdot5\cdot7}{2.4.6.8.6} + \frac{3\cdot5\cdot7\cdot9}{2.4.6.8.10.8}, \&c.$$

$$\frac{3\cdot5\cdot3}{2.4.6.4.2} + \frac{3\cdot5\cdot7\cdot5}{2.4.6.8.6.4} + \frac{3\cdot5\cdot7\cdot9\cdot7}{2.4.6.8.10.8.6} + \frac{3\cdot5\cdot7\cdot9\cdot11\cdot9}{2.4.6.8.10.12.10.8}, \&c.$$

$$\frac{3\cdot5\cdot7\cdot5\cdot3}{2.4.6.8.6.4.2} + \frac{3\cdot5\ 7\cdot9\cdot7\cdot5}{2.4.6.8.10.8.6.4} + \frac{3\cdot5\cdot7\cdot9\cdot11\cdot9\cdot7}{2.4.6.8.10.12.10.8}, \&c.$$

are taken (and they may easily be computed by the method explained in the Philos. Trans. for 1798, p. 547 to 550,) and denoted by the Roman letters a, b, c,* &c. respectively, we shall have

$$d = \begin{cases} A \times : \frac{1}{2} - \frac{3}{2.4.2aa} + \frac{3.5.3}{2.4.6.4.2a^4} - \frac{3.5.7.5.3}{2.4.6.8.6.4.2a^6}, \text{ \&c.} \\ + \sqrt{1 + aa} \times : 1 - \frac{\mathrm{a}}{aa} + \frac{\mathrm{b}}{a^4} - \frac{\mathrm{c}}{a^6}, \text{ \&c.} \end{cases}$$

The law of continuation *ad infinitum* is very evident in the first of this pair of series; and although it is not so in the second, still it is obvious that b is less than a, c than b, &c. *ad infinitum*. And since the numeral coefficients of each of these series may be expressed in decimals, and their logarithms written out ready for use, an arithmetical calculation by this pair of series, when a is considerably greater than 1, will be much easier than by either of the single series given in Art. 25.

But this pair of series may be transformed into another pair converging by the same powers of a, yet of a simpler form, and therefore more convenient for arithmetical calculation. The operations † to be performed on this occasion are as follows.

30. The H. L. of $(\sqrt{1 + aa} - a)$, which enters into the value of A, is $= -$ H. L. $(a + \sqrt{1 + aa})$; and this logarithm, expressed in correct descending series, is $-$ H. L. $2a$, $- \frac{1}{2.2aa} + \frac{3}{2.4.4a^4} - \frac{3.5}{2.4.6.6a^6}$, &c. A, therefore, being $= \frac{1}{a}$ H. L. $(\sqrt{1 + aa} - a)$, is $= - \frac{1}{a}$ H. L. $2a$, $- \frac{1}{2.2a^3} + \frac{3}{2.4.4a^5} - \frac{3.5}{2.4.6.6a^7}$, &c. $= - \frac{1}{a}(\lambda + l) - \frac{1}{2.2a^3} + \frac{3}{2.4.4a^5} - \frac{3.5}{2.4.6.6a^7}$, &c. λ being put for

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* The values of these letters are $\frac{1}{4} + \frac{1}{2}$ H. L. 2, $\frac{11}{64} + \frac{7}{16}$ H. L. 2, and $\frac{17 + 51 \text{ H. L. } 2}{128}$, respectively. See Philos. Trans. for 1798, p. 538.

† See Philos. Trans. for 1798, p. 557 and 558; and for 1800, p. 87 and 88.

H. L. of 2, and l for H. L. of a, for the sake of brevity, and of facility in the comparison of the result of the present operation with one which is next to be made. The multiplication of this series equivalent to A, by its proper factor, may stand thus:

$$\begin{array}{l}
-\frac{1}{a}(\lambda+l)-\frac{1}{2.2a^3}+\frac{3}{2.4.4a^5}-\frac{3.5}{2.4.6.6a^7}, \&c. \\
\qquad\qquad\frac{1}{2}-\frac{3}{2.4.2aa}+\frac{3.5.3}{2.4.6.4.2a^4}-\frac{3.5.7.5.3}{2.4.6.8.6.4.2a^6}, \&c. \\
\hline
-\frac{1}{2a}(\lambda+l)-\frac{1}{2.4a^3}+\frac{3}{4.4.4a^5}-\frac{3.5}{4.4.6.6a^7}, \&c. \\
\qquad\qquad+\frac{3(\lambda+l)}{2.4.2a^3}+\frac{3}{2.4.4.2a^5}-\frac{3.3}{2.4.4.4.4a^7}, \&c. \\
\qquad\qquad\qquad\qquad-\frac{3.5.3(\lambda+l)}{2.4.6.4.2a^5}-\frac{3.5.3}{2.4.4.6.4.2a^7}, \&c. \\
\qquad\qquad\qquad\qquad\qquad\qquad+\frac{3.5.7.5.3(\lambda+l)}{2.4.6.8.6.4.2a^7}, \&c.
\end{array}$$

The value of $\sqrt{1+aa}$ also, in descending series, is $a+\frac{1}{2a}-\frac{1}{2.4.a^3}+\frac{3}{2.4.6.a^5}$, &c. and the multiplication of this series by its proper factor may be made as below:

$$\begin{array}{l}
a+\frac{1}{2a}-\frac{1}{2.4a^3}+\frac{3}{2.4.6a^5}, \&c. \\
1-\frac{\mathrm{a}}{aa}+\frac{\mathrm{b}}{a^4}-\frac{\mathrm{c}}{a^6}, \&c.
\end{array}$$

$$\begin{array}{l}
a+\frac{1}{2a}-\frac{1}{2.4a^3}+\frac{3}{2.4.6a^5}, \&c. \\
\quad-\frac{\mathrm{a}}{a}-\frac{\mathrm{a}}{2a^3}+\frac{\mathrm{a}}{2.4a^5}, \&c. \\
\qquad\qquad+\frac{\mathrm{b}}{a^3}+\frac{\mathrm{b}}{2a^5}, \&c. \\
\qquad\qquad\qquad\quad-\frac{\mathrm{c}}{a^5}, \&c.
\end{array}$$

Now if the series $-\frac{l}{2a}+\frac{3l}{2.4.2a^3}-\frac{3.5.3l}{2.4.6.4.2a^5}$, &c. (which is found in a diagonal line,) be taken from the first of these products, all the coefficients of the remaining terms (since λ = H. L. 2,) will be known quantities; and consequently all the remaining terms in each perpendicular column may be added together, and to their like quantities in the second product; so that the new pair of series expressing the value of d, will be this, viz.

$$\begin{cases} a - \frac{A}{a} + \frac{B}{a^3} \quad - \frac{C}{a^5}, \text{ \&c.} \\ \quad - \frac{l}{2a} + \frac{3l}{2.4.2a^3} - \frac{3.5.3l}{2.4.6\,4\,2a^5}, \text{ \&c.} \end{cases}$$

where the values of A, B, and C, are 0·44314718, 0·05680519, and 0·02183137, respectively. The law which the coefficients of the logarithmic series observe is evident; the law, which the coefficients (A, B, C, &c.) of the other series observe, will be discovered by the following process.

31. It appears by MAC LAURIN'S *Fluxions*, Art. 808, and by the Philos. Trans. for 1802, p. 462, that $d = 1{\cdot}57$, &c. $\times : \frac{aa}{2} - \frac{3a^4}{2.2.4} + \frac{3.3.5a^6}{2.2.4.4.6} - \frac{3.3.5.5.7a^8}{2.2\,4.4.6.6\,8}$, &c.; where it is evident that the value of d depends entirely upon that of a, and that these two quantities must be constant or vary together. Therefore, supposing these quantities to vary, and taking the fluxion on both sides of the equation, we have

$$\dot{d} = 1{\cdot}57 \text{ \&c. } \dot{a} \times : a - \frac{3a^3}{2.2} + \frac{3.3.5a^5}{2.2.4.4} - \frac{3.3.5.5.7a^7}{2.2.4.4.6.6}, \text{ \&c.}$$

This equation divided by a, and the fluxion taken again on both sides, making $\dot{a}$ constant, gives

$$\frac{\ddot{d}}{a} - \frac{\dot{a}\dot{d}}{aa} = 1{\cdot}57 \text{ \&c. } \dot{a}\dot{a}\,{*} : \times - \frac{3a}{2} + \frac{3.3.5a^3}{2.2.4} - \frac{3.3.5.5.7a^5}{2.2.4.4.6}, \text{ \&c.}$$

Here we find the denominators of the fractional coefficients of the terms on the second side of the equation to be the very same as those in the original equation; so that, if we can find herefrom another equation in which the numerators also shall be the same as those which were there found, we shall obtain an important point. Now this may easily be done as follows: Multiply the last equation by a, and take the fluents, and there will be

$$\dot d - \int \frac{\dot a \dot d}{a} = 1{\cdot}57 \text{ \&c. } \dot a \times : -\frac{a^3}{2} + \frac{3.3a^5}{2.2.4} - \frac{3.3.5.5a^7}{2.2.4.4.6}, \text{ \&c.}$$

Divide this equation by a^3, and take the fluents again, and we shall have

$$\int \frac{\dot d}{a^3} - \int \frac{1}{a^3} \int \frac{\dot a \dot d}{a} = 1{\cdot}57 \text{ \&c. } \times : -\frac{a}{2} + \frac{3a^3}{2.2.4} - \frac{3.3.5a^5}{2.2.4.4.6}, \text{ \&c.}$$

which is evidently $= -\frac{d}{a}$.

This new equation will be fitted for our purpose by taking the fluxion on both sides, multiplying by a^3, and then taking the fluxions again; which operations being performed, (remembering that $\dot a$ must still be made constant,) and the terms brought all to one side, and properly arranged, the result will be $\dot a \dot a d + (\frac{1}{a} - a)\dot a \dot d - (aa + 1)\ddot d = 0$.

32. The values of d, $\dot d$, and $\ddot d$, are now to be taken in terms of the pair of series obtained in Art. 30, and substituted in the equation last found. And, although the coefficients of the logarithmic series, and the law which they observe *ad infinitum*, be already discovered, yet, for the sake of brevity, I denote the first, second, third, &c. of them by α, β, γ, &c. respectively. In this notation we have

$$d = \begin{cases} a - \frac{A}{a} + \frac{B}{a^3} - \frac{C}{a^5} + \frac{D}{a^7} - \frac{E}{a^9}, \text{ \&c.} \\ + l \times : \frac{-\alpha}{a} + \frac{\beta}{a^3} - \frac{\gamma}{a^5} + \frac{\delta}{a^7} - \frac{\varepsilon}{a^9}, \text{ \&c.} \end{cases}$$

$$\dot{d} = \begin{cases} \dot{a} \times : 1 + \frac{A}{aa} - \frac{3B}{a^4} + \frac{5C}{a^6} - \frac{7D}{a^8} + \frac{9E}{a^{10}}, \text{ \&c.} \\ + \dot{a} \times : - \frac{\alpha}{aa} + \frac{\beta}{a^4} - \frac{\gamma}{a^6} + \frac{\delta}{a^8} - \frac{\varepsilon}{a^{10}}, \text{ \&c.} \\ + \dot{a}\, l \times : \frac{\alpha}{aa} - \frac{3\beta}{a^4} + \frac{5\gamma}{a^6} - \frac{7\delta}{a^8} + \frac{9\varepsilon}{a^{10}}, \text{ \&c.} \end{cases}$$

$$\ddot{d} = \dot{a}\dot{a} \begin{cases} - \frac{2A}{a^3} + \frac{3.4B}{a^5} - \frac{5.6C}{a^7} + \frac{7.8D}{a^9} - \frac{9.10E}{a^{11}}, \text{ \&c.} \\ + \frac{2\alpha}{a^3} - \frac{4\beta}{a^5} + \frac{6\gamma}{a^7} - \frac{8\delta}{a^9} + \frac{10\varepsilon}{a^{11}}, \text{ \&c.} \\ + \frac{\alpha}{a^3} - \frac{3\beta}{a^5} + \frac{5\gamma}{a^7} - \frac{7\delta}{a^9} + \frac{9\varepsilon}{a^{11}}, \text{ \&c.} \\ + l : \times \frac{-2\alpha}{a^3} + \frac{3.4\beta}{a^5} - \frac{5.6\gamma}{a^7} + \frac{7.8\delta}{a^9} - \frac{9.10\varepsilon}{a^{11}}, \text{ \&c.} \end{cases}$$

The last two equations, more concisely expressed, are

$$\dot{d} = \dot{a} \begin{cases} 1 + \frac{A-\alpha}{aa} - \frac{3B-\beta}{a^4} + \frac{5C-\gamma}{a^6} - \frac{7D-\delta}{a^8} + \frac{9E-\varepsilon}{a^{10}}, \text{ \&c.} \\ + l \times : \frac{\alpha}{aa} - \frac{3\beta}{a^4} + \frac{5\gamma}{a^6} - \frac{7\delta}{a^8} + \frac{9\varepsilon}{a^{10}}, \text{ \&c.} \end{cases}$$

$$\ddot{d} = \dot{a}\dot{a} \begin{cases} - \frac{2A-3\alpha}{a^3} + \frac{3.4B-7\beta}{a^5} - \frac{5.6C-11\gamma}{a^7} + \frac{7.8D-15\delta}{a^9} - \frac{9.10E-19\varepsilon}{a^{11}}, \text{ \&c.} \\ + l \times : \frac{-2\alpha}{a^3} + \frac{3.4\beta}{a^5} - \frac{5.6\gamma}{a^7} + \frac{7.8\delta}{a^9} - \frac{9.10\varepsilon}{a^{11}}, \text{ \&c.} \end{cases}$$

These values of d, $\dot{d}$, and $\ddot{d}$, being written for them in the equation $\dot{a}\dot{a}d + (\frac{1}{a} - a)\, \dot{a}\dot{d} - (aa + 1)\, \ddot{d} = o$, and the algebraic and logarithmic terms severally collected together in two parcels, and the whole divided by aa, we have $o =$

$$\begin{aligned} & a - \frac{A}{a} + \frac{B}{a^3} - \frac{C}{a^5} + \frac{D}{a^7} - \frac{E}{a^9}, \text{ \&c.} \\ & - a - \frac{A-\alpha}{a} + \frac{3B-\beta}{a^3} - \frac{5C-\gamma}{a^5} + \frac{7D-\delta}{a^7} - \frac{9E-\varepsilon}{a^9}, \text{ \&c.} \\ & + \frac{1}{a} + \frac{A-\alpha}{a^3} - \frac{3B-\beta}{a^5} + \frac{5C-\gamma}{a^7} - \frac{7D-\delta}{a^9}, \text{ \&c.} \\ & + \frac{2A-3\alpha}{a} - \frac{3.4B-7\beta}{a^3} + \frac{5.6C-11\gamma}{a^5} - \frac{7.8D-15\delta}{a^7} + \frac{9.10E-19\varepsilon}{a^9}, \text{ \&c.} \\ & + \frac{2A-3\alpha}{a^3} - \frac{3.4B-7\beta}{a^5} + \frac{5.6C-11\gamma}{a^7} - \frac{7.8D-15\delta}{a^9}, \text{ \&c.} \end{aligned}$$

$$+\,l\left\{\begin{array}{rrrrrl}
-\frac{\alpha}{a} & +\frac{\beta}{a^3} & -\frac{\gamma}{a^5} & +\frac{\delta}{a^7} & -\frac{\varepsilon}{a^9}, & \&c. \\
-\frac{\alpha}{a} & +\frac{3\beta}{a^3} & -\frac{5\gamma}{a^5} & +\frac{7\delta}{a^7} & -\frac{9\varepsilon}{a^9}, & \&c. \\
 & +\frac{\alpha}{a^3} & -\frac{3\beta}{a^5} & +\frac{5\gamma}{a^7} & -\frac{7\delta}{a^9}, & \&c. \\
+\frac{2\alpha}{a} & -\frac{3.4\beta}{a^3} & +\frac{5.6\gamma}{a^5} & -\frac{7.8\delta}{a^7} & +\frac{9.10\varepsilon}{a^9}, & \&c. \\
 & +\frac{2\alpha}{a^3} & -\frac{3.4\beta}{a^5} & +\frac{5.6\gamma}{a^7} & -\frac{7.8\delta}{a^9}, & \&c.
\end{array}\right.$$

Now, unless each of these parcels of quantities, as well as their sum, be universally $= o$, this equation will be of no use to us. And if it can be proved, that the coefficients of the terms in the one series are, each of them, $= o$, then it will follow, that each of the coefficients of the terms in the other series is also $= o$. But each of the coefficients of the terms in the logarithmic series is $= o$; which may be proved thus: when the like quantities in this series are added together, the first term will vanish; and the coefficients of the second, third, fourth, fifth, &c. terms (without the common factor l,) will be $3\alpha - 2.4\beta$, $-3.5\beta + 4.6\gamma$, $5.7\gamma - 6.8\delta$, $-7.9\delta + 8.10\varepsilon$, &c. respectively; and the law of continuation is obvious. But by the law (see Art. 29 and 30;) which the coefficients α, β, γ, δ, &c. are known to observe, $\frac{3\alpha}{2.4}$ is $= \beta$, $\frac{3.5\beta}{4.6} = \gamma$, $\frac{5.7\gamma}{6.8} = \delta$, $\frac{7.9\delta}{8.10} = \varepsilon$, &c.; and therefore, $3\alpha - 2.4\beta = o$, $3.5\beta - 4.6\gamma = o$, $5.7\gamma - 6.8\delta = o$, $7.9\delta - 8.10\varepsilon = o$, &c.

The value of α, viz. $\frac{1}{2}$, which was discovered in Art. 29, is found also in the algebraic series, as will presently appear. For, adding like quantities together, the first term of this series also will vanish; and the coefficients of the second, third, fourth, fifth, sixth, &c. will be as follows:

$$\begin{array}{ll}
\text{Coeff. of 2d term} & 1-2\alpha, \\
\text{3d} & -(3.3-1)B+1.3A-4\alpha+6\beta, \\
\text{4th} & +(5.5-1)C-3.5B+8\beta-10\gamma, \\
\text{5th} & -(7.7-1)D+5.7C-12\gamma+14\delta, \\
\text{6th} & +(9.9-1)E-7.9D+16\delta-18\varepsilon, \\
\text{\&c.} & \qquad\text{\&c.}
\end{array}$$

And, putting each of these coefficients $= o$, (since the whole series is now known to be $= o$,) writing 2.4 for its equal $3.3 - 1$, 4.6 for $5.5 - 1$, 6.8 for $7.7 - 1$, &c. we obtain from these equations

$$\alpha = \tfrac{1}{2},$$

$$B = \frac{1.3}{2.4} A - \frac{\alpha}{2} + \frac{3\beta}{2.2},$$

$$C = \frac{3.5}{4.6} B - \frac{\beta}{3} + \frac{5\gamma}{3.4},$$

$$D = \frac{5.7}{6.8} C - \frac{\gamma}{4} + \frac{7\delta}{4.6},$$

$$E = \frac{7.9}{8.10} D - \frac{\delta}{5} + \frac{9\varepsilon}{5.8},$$

&c. &c.

the law of continuation being very evident.

The value of A, which is not discovered by this process, was found in Art. 30, and is $= \frac{1}{4} + \text{H.L.}\,2$. And the decimal values of these coefficients are as below, viz.

$$\begin{aligned}
A &= 0{\cdot}44314718, \\
B &= 0{\cdot}05680519, \\
C &= 0{\cdot}02183137, \\
D &= 0{\cdot}01154452, \\
E &= 0{\cdot}00714200, \\
\text{\&c.} &\quad \text{\&c.}
\end{aligned}$$

33. Thus, by the common application of Sir ISAAC NEWTON's doctrine of fluxions and infinite series, without any assistance

from, or regard to, LANDEN's theorem, we have obtained a pair of series for computing the value of *d*, which converge by the powers of $\frac{1}{aa}$, and of which we can readily find as many terms as we please. And, by a similar process, (as was observed in Art. 26,) may EULER's series for computing the quadrantal arch of an ellipsis be obtained, without any use of FAGNANI's theorem, or the "*tentative methods*," and "*strange artifices*," as Mr. WOODHOUSE * calls them, which appear in EULER's paper.

34. If we look back to Art. 18 and 20 of this Paper, we shall find that, when the transverse and conjugate semi-axes of an hyperbola are denoted by 1 and $\frac{1}{a}$, respectively, (which hyperbola will be similar to that of which the semi-axes are *a* and 1,) the convergency of the first series, derived from LANDEN's theorem, will be by the powers of the fraction $\left(\frac{\frac{1}{a}}{\frac{1}{a}+\sqrt{(1+\frac{1}{aa})}}\right)\frac{1}{1+\sqrt{(aa+1)}}$, assisted by coefficients. And the same rate of convergency will obtain in the series given in Art. 29, by putting $y = \frac{1}{\sqrt{e}}$; for then uu, by which the terms of that series are divided, will be $= 1 + e$, e being $= \sqrt{aa+1}$: so that, when aa is less than $1 + \sqrt{aa+1}$, the terms of the single series will decrease swifter than the terms of the pair of series; and consequently half as many terms of the former as the latter will give a result equally near the truth. The two quantities aa and $1 + \sqrt{aa+1}$ are equal when $aa = 3$; and hence it appears, that the proper use of the pair of series above found, is, when *a* is considerably

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* See Philos. Trans. for 1804, p. 235.

greater than $\surd 3$. When a is a large number, about as many terms of the single series as of the pair must be computed to have the results true to the same number of figures; yet the operation by the pair will be by much the easiest.

35. If both sides of the equation

$$d = \begin{cases} a - \frac{A}{a} + \frac{B}{a^3} - \frac{C}{a^5} + \frac{D}{a^7}, \text{ \&c.} \\ -l \times : \frac{\alpha}{a} - \frac{\beta}{a^3} + \frac{\gamma}{a^5} - \frac{\delta}{a^7}, \text{ \&c.} \end{cases}$$

were divided by a, and if $\frac{d}{a}$ were put $= {'d}$, and $\frac{1}{a} = b$; then, (since H. L. of b would be $= -l$,) we should have

$$'d = \begin{cases} 1 - Abb + Bb^4 - Cb^6 + Db^8, \text{ \&c.} \\ -l \times : \alpha bb - \beta b^4 + \gamma b^6 - \delta b^8, \text{ \&c.} \end{cases}$$

where $'d$ denotes the difference between the lengths of the infinite arch and the asymptote, (and the difference also between the values of the ascending and descending series for computing the arch,) of an hyperbola of which the semi-axes are 1 and b, respectively.

36. It was observed in Art. 12, that the fluxion there given, of an hyperbolic arch, is as capable of transformation as that which has been commonly used for the rectification of an ellipsis: so also are those which I have used in the Philos. Trans. for 1802, p. 451 and 455, and from which series converging by the powers of $\left(\frac{c-a}{c+a}\right)^2$, &c. may easily be obtained; but to treat of such transformations is not only foreign from my present design, but would extend this paper to a considerable length. I shall therefore only point out, by a few examples, the great advantage, in many cases, of computing by descending series, and then conclude.

37. *Example* 1. The transverse and conjugate semi-axes

of an hyperbola are 7 and 1, respectively; it is required to find the lengths of three arches of which the three ordinates are 10, 20, and 30.

Setting aside the circuitous method of rectifying the hyperbola by means of two ellipses, if one was to think of computing these arches by means of a similar hyperbola, and by the theorem (δ) given in Art. 9, (which is a very useful *Formula* within the limits specified in Art. 13,) he would quickly perceive, that the ascending series, by which the value of G is obtained, would converge very slowly; and therefore would make choice of some other method. Theorem IV. in my former tract (see Philos. Trans. for 1802, p. 454,) is a very convenient form to be used on this occasion, and is as follows:

Retaining the notation used in the beginning of Art. 29,

$$z = \begin{cases} e\sqrt{yy+1} \\ + \frac{1}{2e}A - \frac{1}{2.4e^3}B + \frac{3}{2.4.6e^5}C - \frac{3.5}{2.4.6.8e^7}D, \text{ \&c.} \\ - d; \end{cases}$$

$$\text{where } A = \text{H. L. } \frac{\sqrt{(yy+1)}-1}{y},$$

$$B = \frac{-\sqrt{(yy+1)}}{2yy} - \frac{A}{2},$$

$$C = \frac{-\sqrt{(yy+1)}}{4y^4} - \frac{3B}{4},$$

$$D = \frac{-\sqrt{(yy+1)}}{6y^6} - \frac{5C}{6},$$

&c. &c.

The first part of the work may be to find the value of d, which may easily be done by the pair of series given in Art. 32; but, since a computation of it was made by two series, in p. 466, 467, and 468 of the volume here referred to, the ascending series being = 0·6360768, the descending series

$= 7{\cdot}4349912$, and their difference, d, $= 6{\cdot}7989144$; we need only to verify that work, which may easily be done by the *Formulæ* given in Art. 25 and 27, thus: denoting the value of the ascending series corresponding to the ordinate $\frac{1}{\sqrt{e}}$ by ζ, and the value of the descending series corresponding to the same ordinate by S, we have,

$$\text{by Art. 25, } d = 1 + e - 2\zeta, \left\{\begin{array}{lr} = & 8{\cdot}0710678 \\ - & 1{\cdot}2721536 \end{array}\right\}$$

$$= \quad 6{\cdot}7989142;$$

$$\text{by Art. 27, } d = 2S - 1 - e, \left\{\begin{array}{lr} = & 14{\cdot}8699824 \\ - & 8{\cdot}0710678 \end{array}\right\}$$

$$= \quad 6{\cdot}7989146;$$

the difference in the last figures of the results arising from the inaccuracy of decimal fractions, and being wholly inconsiderable.

The rest of the work may stand as follows: when y is $= 10$, then, (a being $= 7$, and $= \sqrt{aa + 1} = \sqrt{50}$,)

$$A = \text{H. L. } \frac{\sqrt{(yy+1)} - 1}{y} = -\, 0{\cdot}0998341,$$

$$B = \frac{-\sqrt{(yy+1)}}{2yy} - \frac{A}{2} = -\, 0{\cdot}0003323,$$

$$C = \frac{-\sqrt{(yy+1)}}{4y^4} - \frac{3B}{4} = -\, 0{\cdot}0000020;$$

of which terms two only are wanted to obtain a result true to seven places of figures. Hence we have

+	—
$e\sqrt{yy+1} = 71{\cdot}0633520,$	$+\frac{1}{2e}A = 0{\cdot}0070593;$
$-\frac{1}{2.4e^3}B = 0{\cdot}0000001,$	$-d = 6{\cdot}7989144,$
sum of the posit. terms $71{\cdot}0633521$;	the sum $-6{\cdot}8059737$;
neg. term, & correc. $-$ $6{\cdot}8059737$;	

the difference is $64{\cdot}2573784$ = the length of the first arch.

When $y = 20$, we have

$$A = \text{H. L.} \frac{\sqrt{(yy+1)}-1}{y} = -0{\cdot}0499794,$$

$$B = \frac{\sqrt{(yy+1)}}{2yy} - \frac{A}{2} = -0{\cdot}0000415;$$

of which two terms the first only is sufficient to give the result true to eight places of figures. We now have

+	—
$e\sqrt{yy+1} = 141{\cdot}5980226,$	$+\frac{1}{2e}A = 0{\cdot}0035341,$
$-\frac{1}{2.4e^3}B = 0{\cdot}0000000,$	$-d = 6{\cdot}7989144,$
sum of the posit. terms $141{\cdot}5980226$;	the sum $-6{\cdot}8024485$;
neg. term & correc. $-$ $6{\cdot}8024485$;	

the difference is $134{\cdot}7955741$ = the length of the second arch.

When y is $= 30$, the value of $A = \text{H. L.} \frac{\sqrt{(yy+1)}-1}{y}$ is $-0{\cdot}0333274$; and since the value of B (as appeared in the preceding operation,) need not be computed, we have

+	—
$e\sqrt{yy+1} = 212{\cdot}2498528,$	$\frac{1}{2e}A = 0{\cdot}0023566,$
neg. term and correc. $-$ $6{\cdot}8012710,$	$-d = 6{\cdot}7989144;$
length of the third arch $205{\cdot}4485818$.	sum $6{\cdot}8012710$;

It is now obvious that, to obtain the length of any greater arch of this hyperbola, the values of the algebraic quantity $e\sqrt{yy+1}$, and $\frac{1}{2e}$ A, the first term of the series, are all that need be computed; for the value of d, once found, serves for all. And if seven more arches of this hyperbola, corresponding to the ordinates 40, 50, 60, &c. to 100, were to be computed, this theorem would afford a striking instance of the great utility of descending series.

38. A *second example* might be, to find the lengths of ten arches of an equilateral hyperbola, of which the semi-axis is 1, when the ordinates are 1, 2, 3, 4, 5, 6, 7, 8, 9, and 10.

These arches may be computed by the new theorem given in Art. 9, in which the value of G is always given in ascending series; but that series, when y is much greater than 1, will converge very little faster than the powers of $\frac{1}{2}$: whereas, by using the theorem given in the Philos. Trans. for 1802, p. 458, viz.

$$z = \begin{cases} u - \frac{1}{2.3u^3} - \frac{3}{2.4.7u^7} - \frac{3.5}{2.4.6.11u^{11}} - \frac{3.5.7}{2.4.6.8.15u^{15}}, \text{ \&c.} \\ - d, \end{cases}$$

(where z denotes the arch of an equilateral hyperbola of which the semi-axis is 1, u is $= \sqrt{1+2yy}$, and d is $= 0.59907012$;) the geometrical progressions which will have place in the series, for the respective ordinates, will be the powers of these fractions, viz. $\frac{1}{9}$, $\frac{1}{81}$, $\frac{1}{361}$, $\frac{1}{1089}$, $\frac{1}{2601}$, $\frac{1}{5329}$, $\frac{1}{9801}$, $\frac{1}{16641}$, $\frac{1}{26569}$, and $\frac{1}{40401}$.

39. In these examples the use and advantage of descending series appear: more examples of their utility might be given; and it might easily be shown, that there are cases in which

such series have the advantage, even when the ascending series have a good rate of convergency. I trust, however, that enough has been done in this Paper to satisfy all candid and competent judges of the matter, that *the Rectification of the Hyperbola by means of two Ellipses is more curious than useful;* that the advantage of computing by descending series, is, in many cases, very great; and that such series will often answer the end of a transformation without the trouble of making it.

VIII. *On a Combination of Oxymuriatic Gas and Oxygene Gas. By* Humphry Davy, *Esq. LL.D. Sec. R. S. Prof. Chem. R. I.*

Read February 21, 1811.

I SHALL beg permission to lay before the Society the account of some experiments on a compound of oxymuriatic gas and oxygene gas, which, I trust, will be found to illustrate an interesting branch of chemical enquiry, and which offer some extraordinary and novel results.

I was led to make these experiments in consequence of the difference between the properties of oxymuriatic gas prepared in different modes; it would occupy a great length of time to state the whole progress of this investigation. It will, I conceive, be more interesting that I should immediately refer to the facts; most of which have been witnessed by Members of this Body, belonging to the Committee of Chemistry of the Royal Institution.

The oxymuriatic gas prepared from manganese, either by mixing it with a muriate and acting upon it by sulphuric acid, or by mixing it with muriatic acid, is when the oxide of manganese is pure, and, whether collected over water or mercury, uniform in its properties; its colour is a pale yellowish green; water takes up about twice its volume; and scarcely gains any colour; the metals burn in it readily; it combines with hydrogene without any deposition of moisture: it does not act on nitrous gas or muriatic acid, or carbonic oxide, or sulphureous gasses, when they have been carefully dried. It is the substance which I employed in all the experiments on the combinations of oxymuriatic gas, described in my last two papers.

The gas produced by the action of muriatic acid on the salts which have been called hyperoxymuriates, on the contrary, differs very much in its properties, according as the manner in which it is prepared and collected is different.

When much acid is employed to a small quantity of salt, and the gas is collected over water, the water becomes tinged of a lemon colour; but the gas collected is the same as that procured from manganese.

When the gas is collected over mercury, and is procured from a weak acid, and from a great excess of salt, by a low heat, its colour is a dense tint of brilliant yellow green, and it possesses properties entirely different from the gas collected over water.

It sometimes explodes during the time of its transfer from one vessel to another, producing heat and light, with an expansion of volume; and it may be always made to explode by a very gentle heat, often by that of the hand.*

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* My brother, Mr. J. DAVY, from whom I receive constant and able assistance in all my chemical enquiries, had several times observed explosions, in transferring the gas from hyperoxymuriate of potash, over mercury, and he was inclined to attribute the phænomenon to the combustion of a thin film of mercury, in contact with a globule of gas. I several times endeavoured to produce the effect, but without success, till an acid was employed for the preparation of the gas, so diluted as not to afford it without the assistance of heat The change of colour and expansion of volume, when the effect took place, immediately convinced me, that it was owing to a decomposition of the gas.

It is a compound of oxymuriatic gas and oxygene, mixed with some oxymuriatic gas. This is proved by the results of its spontaneous explosion. It gives off, in this process, from $\frac{1}{6}$ to $\frac{2}{5}$ its volume of oxygene, loses its vivid colour, and becomes common oxymuriatic gas.

I attempted to obtain the explosive gas in a pure form, by applying heat to a solution of it in water; but in this case, there was a partial decomposition; and some oxygene was disengaged, and some oxymuriatic gas formed. Finding that in the cases when it was most pure, it scarcely acted upon mercury, I attempted to separate the oxymuriatic gas with which it is mixed, by agitation in a tube with this metal; corrosive sublimate formed, and an elastic fluid was obtained, which was almost entirely absorbed by $\frac{1}{4}$ of its volume of water.

This gas in its pure form is so easily decomposable, that it is dangerous to operate upon considerable quantities.

In one set of experiments upon it, a jar of strong glass, containing 40 cubical inches, exploded in my hands with a loud report, producing light; the vessel was broken, and fragments of it were thrown to a considerable distance.

I analysed a portion of this gas, by causing it to explode over mercury in a curved glass tube, by the heat of a spirit lamp.

The oxymuriatic gas formed, was absorbed by water; the oxygene was found to be pure, by the test of nitrous gas.

50 parts of the detonating gas, by decomposition, expanded so as to become 60 parts. The oxygene, remaining after the absorption of the oxymuriatic gas, was about 20 parts. Several other experiments were made, with similar results. So that it may be inferred, that it consists of 2 in volume of oxymuriatic gas, and 1 in volume of oxygene; and the oxy-

gene in the gas is condensed to half its volume. Circumstances conformable to the laws of combination of gaseous fluids, so ably illustrated by M. GAY LUSSAC, and to the theory of definite proportions.

I have stated on a former occasion, that approximations to the numbers representing the proportions in which oxygene and oxymuriatic gas combine, are found in 7.5 and 32.9. And this compound gas contains nearly these quantities.*

The smell of the pure explosive gas somewhat resembles that of burnt sugar, mixed with the peculiar smell of oxymuriatic gas. Water appeared to take up eight or ten times its volume; but the experiment was made over mercury, which might occasion an error, though it did not seem to act on the fluid. The water became of a tint approaching to orange.

When the explosive gas was detonated with hydrogene, equal to twice its volume, there was a great absorption, to more than $\frac{1}{3}$, and solution of muriatic acid was formed; when the explosive gas was in excess, oxygene was always expelled, a fact demonstrating the stronger attraction of hydrogene for oxymuriatic gas than for oxygene.

I have said that mercury has no action upon this gas in its purest form at common temperatures. Copper and antimony, which so readily burn in oxymuriatic gas, did not act upon the explosive gas in the cold: and when they were introduced into it, being heated, it was instantly decomposed, and its oxygene set free; and the metals burnt in the oxymuriatic gas.

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* In page 245 of the Phil. Trans. for 1810, I have mentioned that the specific gravity of oxymuriatic gas, is between 74 and 75 grains per 100 cubical inches. The gas that I weighed, was collected over water and procured from hyperoxymuriate of potash, and at that time I conceived, that this elastic fluid did not differ from the oxymuriatic gas from manganese, except in being purer. It probably contained some of the new gas; for I find that the specific gravity of pure oxymuriatic gas from manganese, and muriatic acid is to that of common air, as 244 to 100 Taking this estimation, the specific gravity of the new gas will be about 238, and the number representing the proportion in which oxymuriatic gas combines, from this estimation, will be rather higher than is stated above.

When sulphur was introduced into it, there was at first no action, but an explosion soon took place: and the peculiar smell of oxymuriate of sulphur was perceived,

Phosphorus produced a brilliant explosion, by contact with it in the cold, and there was produced phosphoric acid and solid oxymuriate of phosphorus.

Arsenic introduced into it did not inflame; the gas was made to explode, when the metal burnt with great brilliancy in the oxymuriatic gas.

Iron wire introduced into it did not burn, till it was heated so as to produce an explosion, when it burnt with a most brilliant light in the decomposed gas.

Charcoal introduced in it ignited, produced a brilliant flash of light, and burnt with a dull red light, doubtless owing to its action upon the oxygene mixed with the oxymuriatic gas.

It produced dense red fumes when mixed with nitrous gas, and there was an absorption of volume.

When it was mixed with muriatic acid gas, there was a gradual diminution of volume. By the application of heat the absorption was rapid, oxymuriatic gas was formed, and a dew appeared on the sides of the vessel.

These experiments enable us to explain the contradictory accounts that have been given by different authors of the properties of oxymuriatic gas.

That the explosive compound has not been collected before, is owing to the circumstance of water having been used for receiving the products from hyperoxymuriate of potash, and unless the water is highly saturated with the explosive gas, nothing but oxymuriatic gas is obtained; or to the circumstance of too dense an acid having been employed.

This substance produces the phænomena which Mr. Chenevix, in his able paper on oxymuriatic acid, referred to the hyperoxygenised muriatic acid; and they prove the truth of his ideas respecting the possible existence of a compound of oxymuriatic gas, and oxygene in a separate state.

The explosions produced in attempts to procure the products of hyperoxymuriate of potash by acids are evidently owing to the decomposition of this new and extraordinary substance.

All the conclusions which I have ventured to make respecting the undecompounded nature of oxymuriatic gas, are, I conceive, entirely confirmed by these new facts.

If oxymuriatic gas contained oxygene, it is not easy to conceive, why oxygene should be afforded by this new compound to muriatic gas, which must already contain oxygene in intimate union. Though on the idea of muriatic acid being a compound of hydrogene and oxymuriatic gas, the phænomena are such as might be expected.

If the power of bodies to burn in oxymuriatic gas depended upon the presence of oxygene, they all ought to burn with much more energy in the new compound; but copper and antimony, and mercury, and arsenic, and iron, and sulphur have no action upon it, till it is decomposed; and they act then according to their relative attractions on the oxygene, or on the oxymuriatic gas.

There is a simple experiment which illustrates this idea;

Let a glass vessel containing brass foil be exhausted, and the new gas admitted, no action will take place; throw in a little nitrous gas, a rapid decomposition occurs, and the metal burns with great brilliancy.

Supposing oxygene and oxymuriatic gas to belong to the same class of bodies; the attraction between them might be conceived very weak, as it is found to be, and they are easily separated from each other, and made repulsive by a very low degree of heat.

The most vivid effects of combustion known, are those produced by the condensation of oxygene or oxymuriatic gas; but in this instance, a violent explosion with heat and light are produced by their separation, and expansion, a perfectly novel circumstance in chemical philosophy.

This compound destroys dry vegetable colours, but first gives them a tint of red. This and its considerable absorbability by water would incline one to adopt Mr. CHENEVIX's idea that it approaches to an acid in its nature. It is probably combined with the peroxide of potassium in the hyperoxymuriate.

That oxymuriatic gas and oxygene combine and separate from each other with such peculiar phænomena, appears strongly in favour of the idea of their being distinct, though analogous species of matter. It is certainly possible to defend the hypothesis that oxymuriatic gas consists of oxygene united to an unknown basis; but it would be possible likewise to defend the speculation that it contains hydrogene.

Like oxygene it has not yet been decomposed; and I sometime ago made an experiment, which, like most of the others I have brought forward, is very adverse to the idea of its containing oxygene.

I passed the solid oxymuriate of phosphorus in vapour, and oxygene gas together through a green glass tube heated to redness.

A decomposition took place, and phosphoric acid was formed, and oxymuriatic gas was expelled.

Now, if oxygene existed in the oxymuriate of phosphorus, there is no reason why this change should take place. On the idea of oxymuriatic gas being undecompounded, it is easily explained. Oxygene is known to have a stronger attraction for phosphorus than oxymuriatic gas has, and consequently ought to expel it from this combination.

As the new compound in its purest form is possessed of a bright yellow green colour, it may be expedient to designate it by a name expressive of this circumstance, and its relation to oxymuriatic gas. As I have named that elastic fluid Chlorine, so I venture to propose for this substance the name Euchlorine, or Euchloric gas from ευ and χλωρος. The point of Nomenclature I am not, however, inclined to dwell upon. I shall be content to adopt any name that may be considered as most appropriate by the able chemical philosophers attached to this Society.

⁂ In page 27, line 11, of the Bakerian lecture, for "water separated and LIBAVIUS's liquor was formed," read "a compound of water and LIBAVIUS's liquor separated." In page 21, it is stated that magnesia is not decomposed by oxymuriatic gas at a red heat. From some experiments of M. M. GAY LUSSAC, and THENARD, Bullet. de la Societ. Phil. Mai, 1810, it appears that oxygene is procured by passing oxymuriatic gas over magnesia, at a high temperature, and that a muriate indecomposable by heat is proved. They attribute the presence of this oxygene to the decomposition of the acid, but according to all analogies, it must arise from the decomposition of the earth.

VIII. *Experiments to prove that Fluids pass directly from the Stomach to the Circulation of the Blood, and from thence into the Cells of the Spleen, the Gall Bladder, and Urinary Bladder, without going through the Thoracic Duct. By* Everard Home, *Esq. F. R. S.*

Read January 31, 1811.

HAVING on a former occasion laid before the Society some experiments, to prove that fluids pass directly from the cardiac portion of the stomach, so as to arrive at the circulation of blood without going through the thoracic duct, the only known channel by which liquids can arrive there; the present experiments are brought to confirm that opinion; but in stating them, I wish to correct an error I was led into, in believing that the spleen was the channel, by which they are conveyed.

At the time I made my former communications, I was conscious that the facts I had ascertained were only sufficient to open a new field of enquiry; but as I might never be able to make a further progress in an investigation, beset with so many difficulties, I thought it right to put them on record. Since that time I have lost no opportunity of devising new experiments to elucidate this subject; and the circumstance of Mr. BRODIE, the assistant of my philosophical as well as professional labours, having tied the thoracic duct in some experiments which will come before the Society, suggested to me the idea, that if the thoracic duct was tied, and proper experiments made, there could be no difficulty in ascertaining whether there was any other channel between the stomach and the circulation of the blood.

With this view I instituted the following experiment, which was made on the 29th of September 1810, by Mr. BRODIE, assisted by Mr. WILLIAM BRANDE and Mr. GATCOMBE. I was unavoidably prevented from being present during the time of the experiment.

Experiment 1.

A ligature was passed round the thoracic duct of a rabbit, just before it enters at the junction between the left jugular and subclavian veins: an ounce of strong infusion of rhubarb was then injected into the stomach. In three quarters of an hour some urine was voided, in which rhubarb was distinctly detected, by the addition of potash. An hour and a quarter after the injection of the rhubarb the animal was killed: a dram and half of urine was found in the bladder highly tinged with rhubarb, and the usual alteration of colour took place on the addition of potash. The coats of the thoracic duct had given way opposite the middle dorsal vertebra, and nearly an ounce of chyle was found effused into the cavity of the thorax, beside a considerable quantity in the cellular membrane of the posterior mediastinum. Above the ruptured part the thoracic duct was entire, much distended with chyle; and on tracing it upwards, the termination of the duct in the vein was found to be completely secured by the ligature. The lacteal and lymphatic vessels had given way in several parts of the abdomen, and chyle and lymph were extravasated underneath the peritoneum.

In this and the following experiments the infusion of rhubarb was employed in preference to the prussiate of potash, in consequence of its having been found in those I formerly made, that one drop of tincture of rhubarb could be detected in half an ounce of serum, and nothing less than a quarter of a grain of prussiate of potash in the same quantity could be made to strike a blue colour when the test was added.

Experiment 2.

The experiment was repeated upon a dog. In this I was assisted by Mr. BRODIE, Mr. WILLIAM BRANDE, Mr. CLIFT, and Mr. GATCOMBE. After the thoracic duct had been secured, two ounces of strong infusion of rhubarb were injected into the stomach, and in an hour the dog was killed. The urine in the bladder, on the addition of potash, became deeply tinged with rhubarb. The bile in the gall bladder, by a similar test, was found to contain rhubarb. The lacteal vessels in several parts of the mesentery had burst, and chyle was extravasated into the cellular membrane; the thoracic duct had given way in the lower part of the posterior mediastinum, and chyle was extravasated. Above the ruptured part the thoracic duct was much distended with chyle; it was readily traced to the ligature, by which it was completely secured.

These experiments appeared to establish the fact, that the thoracic duct was not the channel through which the infusion of rhubarb was conveyed to the circulation of the blood, and it now became easy to ascertain, whether it passed through the spleen, by extirpating that organ, and repeating the last experiment.

On the 21st of October, 1810, the following experiment, was made with the assistance of Mr. BRODIE, Mr. CLIFT, Mr. GATCOMBE, and Mr. MONEY.

Experiment 3.

The thoracic duct near its termination was secured in a dog, whose spleen had been removed four days before, and three ounces of infusion of rhubarb were injected into the stomach: in an hour and half the dog was killed, and the urine was found strongly impregnated with rhubarb; and on examination, the thoracic duct was found to be completely secured by the ligature. Several of the lacteals had burst, but the duct itself had not given way; it was greatly distended with chyle and lymph.

By this experiment it was completely ascertained, that the spleen is not the channel through which the infusion of rhubarb is conveyed into the circulation of the blood, as I had been led to believe, and therefore the rhubarb, in my former experiments detected in the spleen, must have been deposited there in the same manner as in the urine, and in the bile.

The detection of this error made me more anxious to avoid being misled respecting the thoracic duct; and therefore, although there was little probability that the infusion of rhubarb could have passed into the lymphatic vessels, which open into the blood vessels of the right side of the neck, I thought it right, before I proceeded further, to repeat the experiment, securing the termination of the thoracic duct on the left side, and the lymphatic trunk of the right side, where it empties itself into the angle between the jugular and subclavian vein. This was done on the 28th of October, 1810, with the assistance of the same persons as in the last experiment.

Experiment 4.

The thoracic duct of a dog was tied, as in the former experiment; in doing it the duct was wounded, and about a dram of chyle flowed out; the lymphatic trunk of the right side was then secured. After this, three ounces of infusion of rhubarb were injected into the stomach, and in an hour the dog was killed. The urine and the bile were found distinctly impregnated with rhubarb. On opening the thorax, some absorbent vessels, distended with lymph, were seen on the right side of the spine, entering an absorbent gland on the second dorsal vertebra, and the vasa efferentia from the gland were seen uniting with other absorbent vessels, and extending towards the right shoulder, where they formed a common trunk with the absorbents from the neck and axilla; this trunk was found included in the ligature. The thoracic duct was moderately distended with a mixture of chyle and lymph; in tracing it upwards, an opening was seen in it immediately below the ligature, through which the contents readily passed out when pressure was made on the duct: above this opening the duct was completely secured by the ligature. Nearly a dram of the fluid contained in the thoracic duct was collected and tested by potash, but there did not appear to be any impregnation of rhubarb.

Experiment 5.

The last experiment was repeated on another dog, on the 21st of January, 1811, with the assistance of Mr. BRODIE, Mr. W. BRANDE, Mr. CLIFT, and Mr. GATCOMBE. The dog was killed an hour after the thoracic duct and lymphatic trunk had been secured, and the infusion of rhubarb had been injected into the stomach.

In tying the right lymphatic trunk, a lymphatic vessel, from the thorax going to join it, was wounded, from which chyle flowed out in considerable quantity during the whole time of the experiment; a short time before the dog was killed some of it was collected, but on testing it with potash no rhubarb was detected in it.

The urine was found impregnated with rhubarb, as was also the bile from the gall bladder; but both in a less degree than in the last experiment. The lacteal vessels and mesenteric glands were much distended with chyle; and on cutting into the glands chyle flowed out in considerable quantity. Some of this was collected and tested with potash, but shewed no evidence of rhubarb being contained in it. The thoracic duct was much distended; it was traced to the ligature, and was found to be completely secured.

Lymphatic vessels from the right side of the posterior mediastinum, were seen extending towards the ligature that had been tied on that side; they were nearly empty; and the trunk formed by the junction of these with the lymphatic vessels from the right axilla, and from the right side of the neck, was seen distinctly included in the ligature.

While Mr. BRODIE was tracing the thoracic duct, Mr. WILLIAM BRANDE was making an infusion of the spleen, and shewed me a section of it, in which the cells were larger, and more distinct, than I had ever seen them in a dog. There was a slight tinge of rhubarb in the infusion from the spleen. A similar infusion was made of the liver; but the quantity of blood contained in it being much greater than in the spleen, the appearance was not sufficiently distinct to decide whether it contained rhubarb or not. These experiments appear completely to establish the fact, that the rhubarb did not pass through the thoracic duct, and therefore must have got into the circulation of the blood by some other channel. They likewise completely overturn the opinion I had adopted of the spleen being the medium by which the rhubarb had been conveyed, and show that the spleen answers some other purposes in the animal economy.

The rhubarb found in the spleen does not arrive there before it enters the circulation, it is therefore most probably afterwards deposited in the cells in the form of a secretion. That the rhubarb goes into the circulation is proved by my former experiments, in which it was detected in the splenic vein. The prussiate of potash is hardly to be discovered in the blood of a living animal, since the proportion which strikes a blue colour on the addition of solution of iron, is greater than the circulating fluids can be expected to contain at any one time, as it goes off by the secretions nearly as fast as it is received into the blood vessels. In a moderately sized ass more than two drams must be dissolved in the blood before its presence there can be detected.

That the fluid contained in the cells of the spleen is secreted there, is rendered highly probable, since it is most abundant while the digestive organs are employed, and scarcely at all met with when the animal has been sometime without food. The great objection to this opinion is, there being no excretory duct but the lymphatic vessels of the spleen; these however are both larger and more numerous than in any other organ; they are found in the ass to form one common trunk, which opens into a large gland on the side of the thoracic duct, just above the receptaculum chyli; and when the quicksilver is made to pass through the branches of this gland, there is a trunk equally large on the opposite side, which makes an angle, and then terminates in the thoracic duct. This fact I ascertained at the Veterinary College, assisted by the Deputy Professor Mr. SEWELL, and Mr. CLIFT. These lymphatic vessels are equally large as the excretory ducts of any other glands, and therefore sufficient to carry off the secretion formed in the cells of the spleen ; and where a secretion is to be carried into the thoracic duct, it would be a deviation from the general plan of the animal economy, were any but lymphatic vessels employed for that purpose.

It is a strong circumstance in favour of the secretion being so conveyed, that in the last experiment, the lacteals and cells of the spleen were unusually turgid, being placed under similar circumstances, the thoracic duct being so full as not to receive their contents.

The purposes that are answered by such a secretion from the spleen into the thoracic duct cannot at present be ascertained.

IX. *On the Composition of Zeolite. By* James Smithson, *Esq. F.R.S.*

Read February 7, 1811.

Mineral bodies being, in fact, *native chemical preparations*, perfectly analogous to those of the laboratory of art, it is only by chemical means, that their species can be ascertained with any degree of certainty, especially under all the variations of mechanical state and intimate admixture with each other, to which they are subject.

And accordingly, we see those methods which profess to supersede the necessity of chemistry in mineralogy, and to decide upon the species of it by other means than her's, yet bringing an unavoidable tribute of homage to her superior powers, by turning to her for a solution of the difficulties which continually arise to them, and to obtain firm grounds to relinquish or adopt the conclusions to which the principles they employ, lead them.

Zeolite and natrolite have been universally admitted to be species distinct from each other, from Mr. Klaproth having discovered a considerable quantity of soda and no lime, in the composition of the latter, while Mr. Vauquelin had not found any portion of either of the fixed alkalies, but a considerable one of lime, in his analysis of zeolite.*

The natrolite has been lately met with under a regular crystalline form, and this form appears to be perfectly similar to that of zeolite, but Mr. HAÜY has not judged himself warranted by this circumstance, to consider these two bodies as of the same species, because zeolite, he says, " does not contain an atom of soda." *

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* Journal des Mines, No. XLIV.

I had many years ago found soda in what I considered to be zeolites, which I had collected in the island of Staffa, having formed GLAUBER's salt by treating them with sulphuric acid; and I have since repeatedly ascertained the presence of the same principle in similar stones from various other places; and Dr. HUTTON and Dr. KENNEDY, had likewise detected soda in bodies, to which they gave the name of zeolite.

There was, however, no certainty that the subjects of any of these experiments were of the same nature as what Mr. VAUQUELIN had examined, were of that species which Mr. HAÜY calls mesotype.

Mr. HAÜY was so obliging as to send me lately, some specimens of minerals. There happened to be amongst them a cluster of zeolite in rectangular tetrahedral prisms, terminated by obtuse tetrahedral pyramids whose faces coincided with those of the prism. These crystals were of a considerable size, and perfectly homogeneous, and labelled by himself "*Mesotype pyramidée du depart. du Puy de Dôme.*" I availed myself of this very favourable opportunity, to ascertain whether the mesotype of Mr. HAÜY and natrolite, did or did not differ in their composition, and the results of the experiments have been entirely unfavourable to their separation, as the following account of them will show.

10 grains of this zeolite being kept red hot for five minutes

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* Journal des Mines, No. CL. Juin 1810, p. 458.

lost 0.75 grains, and became opaque and friable. In a second experiment, 10 grains being exposed for 10 minutes to a stronger fire, lost 0.95 grains, and consolidated into a hard transparent state.

10 grains of this zeolite, which had not been heated, were reduced to a fine powder, and diluted muriatic acid poured upon it. On standing some hours, without any application of heat, the zeolite entirely dissolved, and some hours after, the solution became a jelly: this jelly was evaporated to a dry state, and then made red hot.

Water was repeatedly poured on to this ignited matter till nothing more could be extracted from it. This solution was gently evaporated to a dry state, and this residuum made slightly red hot. It then weighed 3.15 grains. It was *muriate of soda.*

The solution of this muriate of soda being tried with solutions of carbonate of ammonia and oxalic acid, did not afford the least precipitate, which would have happened had the zeolite contained any lime, as the muriate of lime* would not have been decomposed by the ignition.

The remaining matter, from which this muriate of soda had been extracted, was repeatedly digested with marine acid, till all that was soluble was dissolved. What remained was silica, and, after being made red hot, weighed 4.9 grains.

The muriatic solution, which had been decanted off from the silica, was exhaled to a dry state, and the matter left made red hot. It was alumina.

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* These names are retained for the present, as being familiar, though, since Mr. DAVY's important discovery of the nature of what was called oxymuriatic acid, the substances to which they are applied, are known not to be salts, but metallic compounds analogous to oxides.

To discover whether any magnesia was contained amongst this alumina, it was dissolved in sulphuric acid, the solution evaporated to a dry state, and ignited. Water did extract some saline matter from this ignited alumina, but it had not at all the appearance of sulphate of magnesia, and proved to be some sulphate of alumina which had escaped decomposition, for on an addition of sulphate of ammonia to it, it produced crystals of compound sulphate of alumina and ammonia, in regular octaedrons.

This alum and alumina were again mixed and digested in ammonia, and the whole dried and made red hot. The alumina left, weighed 3.1 grains.

Being suspected to contain still some sulphuric acid, this alumina was dissolved in nitric acid, and an excess of acetate of barytes added. A precipitate of sulphate of barytes fell, which after being edulcorated and made red hot, weighed 1.2 grains. If we admit $\frac{1}{3}$ of sulphate of barytes to be sulphuric acid, the quantity of the alumina will be $= 3.1 - 0.4 = 2.7$ grains.

From the experiments of Dr. Marcet,* it appears that 3.15 grains of muriate of soda, afford 1.7 grains of soda.

Hence, according to the foregoing experiments, the 10 grains of zeolite analysed, consisted of

Silica - - -	4.90
Alumina - -	2.70
Soda - - -	1.70
Ice - - -	0.95
	10.25

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* Phil. Trans. 1807.

As these experiments had been undertaken more for the purpose of ascertaining the nature of the component parts of this zeolite than their proportions, the object of them was considered as accomplished, although perfect accuracy in the latter respect, had not been attained, and which, indeed, the analysis we possess of natrolite by the illustrious chemist of Berlin, renders unnecessary.

I am induced to prefer the name of zeolite for this species of stone, to any other name, from an unwillingness to obliterate entirely from the nomenclature of mineralogy, while arbitrary names are retained in it, all trace of one of the discoveries of the greatest mineralogist who has yet appeared, and which, at the time it was made, was considered as, and was, a very considerable one, being the first addition of an earthy species, made by scientific means, to those established immemorially by miners and lapidaries, and hence having, with tungstein and nickel, led the way to the great and brilliant extension which mineralogy has since received. And, of the several substances, which, from the state of science in his time, certain common qualities induced Baron CRONSTEDT to associate together under the name of zeolite; it is this which has been most immediately understood as such, and whose qualities have been assumed as the characteristic ones of the species.

Indeed, I think that the name imposed on a substance by the discoverer of it, ought to be held in some degree sacred, and not altered without the most urgent necessity for doing it. It is but a feeble and just retribution of respect for the service which he has rendered to science.

Professor STRUVE, of Lausanne, whose skill in mineralogy is well known, having mentioned to me, in one of his letters, that from some experiments of his own, he was led to suspect the existence of phosphoric acid in several stones, and particularly in the zeolite of Auvergne, I have directed my enquiries to this point, but have not found the phosphoric, or any other acknowledged mineral acid, in this zeolite.

Many persons, from experiencing much difficulty in comprehending the combination together of the earths, have been led to suppose the existence of undiscovered acids in stony crystals. If quartz be itself considered as an acid, to which order of bodies its qualities much more nearly assimilate it, than to the earths, their composition becomes readily intelligible. They will then be neutral salts, silicates, either simple or compound. Zeolite will be a compound salt, a hydrated silicate of alumina and soda, and hence a compound of alumina not very dissimilar to alum. And topaz, whose singular ingredients, discovered by Mr. KLAPROTH, have called forth a query from the celebrated Mr. VAUQUELIN, with regard to the mode of their existence together,* will be likewise a compound salt, consisting of silicate of alumina, and fluate of alumina.

Our acquaintance with the composition of the several mineral substances, is yet far too inaccurate to render it possible to point out with any degree of certainty, the one of which zeolite is an hydrate, however the agreement of the two substances in the nature of their constituent parts, and in their being both electrical by heat, directs conjecture towards tourmaline.

St James's Place, Jan. 22, 1811.

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* Annales du Museum d'Hist. Nat. tome 6, p. 24.

Addition to the Account of native Minium.

After I had communicated to the President, the account of the discovery of native minium, printed in the Philosophical Transactions for 1806, I learned that this ore came from the lead mines of Breylau in Westphalia.

X. *Experiments and Observations on the different Modes in which Death is produced by certain vegetable Poisons. By* B. C. Brodie, *Esq. F. R. S. Communicated by the Society for promoting the Knowledge of Animal Chemistry.*

Read February 21, 1811.

I. THE following experiments were instituted with a view to ascertain, in what manner certain substances act on the animal system, so as to occasion death, independently of mechanical injury. I was led to the inquiry, from the subject of it appearing to be of considerable interest and importance, and from a hope, that, in the present improved state of physiological knowledge, we might be enabled to arrive at some more satisfactory conclusions, than had been deduced from any former observations.

The substances, which act as poisons when applied to the animal body are very numerous. In the experiments, which I have hitherto made, I have employed vegetable poisons only. Of these I have selected such, as are very active and certain in producing their effects, believing that, on this account, the exact nature of those effects would be more readily ascertained. The principal objects, which I have kept in view have been to determine, on which of the vital organs the poison employed exercises its primary influence, and through what medium that organ becomes affected. I have also endeavoured to ascertain by what means the fatal consequences of some poisons may be prevented. With some of the conclusions, which I have ventured to draw, so far as I know, we were not before acquainted; and others of them, though not entirely new, had not been previously established by satisfactory experiments.

I shall relate first those experiments, in which poisons were applied internally, that is to the mucous membranes of the tongue or alimentary canal, and afterwards those, in which poisons were applied to wounded surfaces.

II. *Experiments with Poisons applied to the Tongue or alimentary Canal.*

Experiments with Alcohol.

When spirits are taken into the stomach, in a certain quantity, they produce that kind of delirium, which, constitutes intoxication: when taken in a larger quantity, it is well known that, they destroy life altogether, and that in the course of a very short space of time. Intoxication is a derangement of the functions of the mind, and, as these are in some way connected with those of the brain, it seems probable, that it is by acting on this organ, that spirits when taken into the stomach occasion death. In order to ascertain how far this conclusion is just, I made the following experiments.*

Experiment 1. I poured two drams of proof spirits down the œsophagus of a cat. Instantly he struggled violently; then lay on one side, perfectly motionless and insensible; the breathing was laboured and stertorous, and the pulsations of the heart were very frequent. He continued in this state for seven or eight minutes; then began to recover; the respirations became easier, and presently he stood up, and was able to walk.

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* I am indebted to Dr. E N. BANCROFT for his assistance in many of the experiments which I am about to detail. Mr. W. BRANDE lent me his assistance in the greater part of those which were made. I have been further assisted by Mr. BROUGHTON, Mr. R. RAWLINS, and Mr. R. GATCOMBE, and by several other gentlemen.

Exp. 2. I injected an ounce and a half of proof spirits into the stomach of a large full-grown rabbit, by means of an elastic gum tube passed down the œsophagus. The same symptoms took place as in the last experiment; but the animal did not begin to recover from the state of insensibility, until forty minutes had elapsed from the time of the injection.

Exp. 3. Seven drams of proof spirits were injected into the stomach of a younger rabbit. Two minutes afterwards, he evidently was affected by the spirits, and in three minutes more he lay on one side motionless and insensible. The pupils of the eyes were perfectly dilated; there were occasional slight convulsive motions of the extremities; the respiration was laborious, it was gradually performed at longer and longer intervals, and at the end of an hour and fifteen minutes had entirely ceased. Two minutes after the animal was apparently dead, I opened into the thorax, and found the heart acting with moderate force and frequency, circulating dark coloured blood. I introduced a tube into the trachea, and produced artificial respiration by inflating the lungs, and found that by these means the action of the heart might be kept up to the natural standard, as in an animal from whom the head is removed.

Exp. 4. I injected into the stomach of a rabbit two ounces of proof spirits. The injection was scarcely completed, when the animal became perfectly insensible. Precisely the same symptoms took place as in the last experiment, and at the end of twenty-seven minutes, from the time of the injection, the rabbit was apparently dead; but on examining the thorax the heart was found still acting, as in the last experiment.

It has been shewn by M. Bichat, and the observation has been confirmed by some experiments, which I have lately had the honour of communicating to this learned Society, that the brain is not directly necessary to the action of the heart, and that, when the functions of the brain are destroyed, the heart continues to contract for some time afterwards, and then ceases only in consequence of the suspension of respiration, which is under the influence of the brain.

It would appear, from the experiments, which I have just detailed, that the symptoms produced by a large quantity of spirits taken into the stomach, arise entirely from disturbance of the functions of the brain. The complete insensibility to external impressions; the dilatation of the pupils of the eyes; and the loss of motion, indicate that the functions of this organ are suspended; respiration, which is under its influence, is ill performed, and at last altogether ceases; while the heart, to the action of which the brain is not directly necessary, continues to contract, circulating dark coloured blood for some time afterwards.

There is a striking analogy between the symptoms arising from spirits taken internally, and those produced by injuries of the brain.

Concussion of the brain, which may be considered as the slightest degree of injury, occasions a state of mind resembling intoxication, and the resemblance in some instances is so complete, that the most accurate observer cannot form a diagnosis, except from the history of the case. Pressure on the brain, which is a more severe injury than concussion, produces loss of motion, insensibility, dilatation of the pupils; respiration becomes laboured and stertorous, is performed at long intervals, and at last altogether ceases, and the patient dies.

It forms an interesting matter of inquiry, whether spirits when taken into the stomach produce their effects on the brain, by being absorbed into the circulation, or in consequence of the sympathy, that exists between these organs by means of the nerves. The following circumstances lead me to conclude that they act in the last of these two ways.

1. In experiments where animals have been killed by the injection of spirits into the stomach, I have found this organ to bear the marks of great inflammation, but never found any preternatural appearances whatever in the brain. 2. The effects of spirits taken into the stomach in the last experiment were so instantaneous, that it appears impossible that absorption should have taken place before they were produced. 3. A person who is intoxicated, frequently becomes suddenly sober after vomiting. 4. In the experiments, which I have just related, I mixed tincture of rhubarb with the spirits, knowing from the experiments of Mr. Home and Mr. William Brande, that this, when absorbed into the circulation, was readily separated from the blood by the kidneys, and that very small quantities might be detected in the urine by the addition of potash; but, though I never failed to find urine in the bladder, I never detected rhubarb in it.

The including the termination of the thoracic duct in a ligature does not prevent spirits, when taken into the stomach, from producing their usual effects on the nervous system, but subsequent observations, which Mr. HOME has already communicated to this Society, have shewn that no conclusion can be drawn from this experiment.

That a poison may affect a distant organ, through the medium of the nerves, without entering the circulation, is proved by the well-known circumstance of solution of the extract of *Belladonna*, when applied to the tunica conjunctiva of the eye, occasioning dilatation of the pupil of the same eye, though no other part of the system is affected.

It has been formerly supposed by Dr. MEAD and other physiologists, that a poison may produce death by acting on the extremities of the nerves of the stomach and intestines, without being absorbed into the circulation. That it should by these means be capable of affecting the brain is not to be wondered at, when we consider the numerous and various sympathies between this organ and the alimentary canal, evidently independent of any other communication than the nerves.

Experiments with the Essential Oil of Bitter Almonds.[*]

Experiment 5. One drop of the essential oil of bitter almonds was applied to the tongue of a young cat. She was instantly seized with violent convulsions; then lay on one side motionless, insensible, breathing in a hurried manner; the respirations became laboured, took place at longer and longer intervals, and at the end of five minutes, from the application of the poison, had entirely ceased, and the animal was apparently dead; but, on opening the thorax, the heart was found acting regularly eighty times in a minute, circulating dark coloured blood, and it continued to act for six or seven minutes afterwards.

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[*] The essential oil of bitter almonds does not appear to differ from the essential oil of laurel. I was furnished with a quantity of it, first by my friend Mr. WILLIAM BRANDE, and afterwards by Mr. COOKE of Southampton-street.

Exp. 6. I injected into the rectum of a cat half an ounce of water, with two drops of the essential oil. In two minutes afterwards, he was affected with symptoms similar to those, which occurred in the last experiment, and at the end of five minutes, from the injection of the poison, he was apparently dead. Two minutes after apparent death, the heart was found acting eighty times in a minute. On dissection, no preternatural appearances were found either in the internal membrane of the rectum, or the brain.

The symptoms produced by this poison, and the circumstance of the heart continuing to contract after apparent death, lead to the conclusion that it occasions death by disturbing the functions of the brain.

While engaged in these last experiments, I dipped the blunt end of a probe into the essential oil, and applied it to my tongue, meaning to taste it, and having no suspicion that so small a quantity could produce any of its specific effects on the nervous system; but scarcely had I applied it, when I experienced a very remarkable and unpleasant sensation, which I referred chiefly to the epigastric region, but the exact nature of which I cannot describe, because I know nothing precisely similar to it. At the same time there was a sense of weakness in my limbs, as if I had not the command of my muscles, and I thought that I was about to fall. However, these sensations were momentary, and I experienced no inconvenience whatever afterwards.

I afterwards applied a more minute quantity of the essential oil to my tongue several times, without experiencing from it any disagreeable effects; but on applying a larger quantity, I was affected with the same momentary sensations as in the former instance, and there was a recurrence of them in three or four seconds after the first attack had subsided.

From the instantaneousness, with which the effects are produced; and from its acting more speedily when applied to the tongue, than when injected into the intestine, though the latter presents a better absorbing surface, we may conclude that this poison acts on the brain through the medium of the nerves, without being absorbed into the circulation.

Experiment with the Juice of the Leaves of Aconite.

Exp. 7. An ounce of this juice was injected into the rectum of a cat. Three minutes afterwards he voided what appeared to be nearly the whole of the injection; he then stood for some minutes perfectly motionless, with his legs drawn together; at the end of nine minutes, from the time of the injection, he retched and vomited; then attempted to walk, but faultered and fell at every step, as if from giddiness. At the end of thirteen minutes, he lay on one side insensible, motionless, except some slight convulsive motions of the limbs. The respiration became slow and laboured; and at forty-seven minutes from the time of the injection, he was apparently dead. One minute and a half afterwards, the heart was found contracting regularly one hundred times in a minute.

It appears from this experiment, that the juice of aconite, when injected into the intestine, occasions death by destroying the functions of the brain. From the analogy of other poisons, it is rendered probable that it acts on the brain through the medium of the nerves, without being absorbed into the circulation. This opinion is confirmed by the following circumstance: if a small quantity of the leaf of aconite is chewed, it occasions a remarkable sense of numbness of the lips and gums, which does not subside for two or three hours.

Experiments with the Infusion of Tobacco.

Exp. 8. Four ounces of infusion of tobacco were injected into the rectum of a dog. Four minutes afterwards he retched, but did not vomit; he then became faint, and lay motionless on one side; at the end of nine minutes from the time of the injection, the heart could not be felt; he gasped for breath at long intervals; and in another minute there was no appearance whatever of life. I immediately laid open the cavities of the thorax and abdomen. The heart was much distended, and had entirely ceased to contract; there was no peristaltic motion of the intestines.

Exp. 9. An ounce of very strong infusion of tobacco was injected into the rectum of a cat. Symptoms were produced similar to those, which occurred in the last experiment, and the animal died at the end of seven minutes from the time of the injection. On opening the thorax immediately after death, the heart was found extremely distended, and to have entirely ceased acting, with the exception of a slight tremulous motion of the auricles.

Exp. 10. Three ounces of infusion of tobacco were injected into the rectum of a dog. He was affected with symptoms similar to those in the former experiments, and died at the end of ten minutes. On opening the thorax immediately after death, I found the heart much distended, and to have entirely ceased contracting.

Exp. 11. Three ounces of infusion of tobacco were injected into the rectum of a dog. Immediately there took place tremulous contractions of the voluntary muscles. Five minutes afterwards the injection was repeated in the same quantity. The dog then was sick, and threw up some of the infusion, with other matter, from the stomach; he became faint, and died ten minutes after the second injection. Immediately after respiration had ceased, I opened the thorax, and found the heart extremely distended, and without any evident contraction, except of the appendix of the right auricle, which every now and then contracted in a slight degree. I divided the pericardium on the right side. In consequence of the extreme distension of the heart, this could not be done without irritating the fibres with the point of the scalpel. Immediately both auricles and ventricles began to contract with considerable force, so as to restore the circulation. Artificial respiration was produced, and the circulation was kept up for more than half an hour, beyond which time the experiment was not continued.

We may conclude from these experiments, that the effect of the infusion of tobacco, when injected into the intestine of a living animal, is to destroy the action of the heart, stopping the circulation and producing syncope. It appeared to me that the action of the heart ceased even before the animal had ceased to respire; and this was confirmed by another experiment, in which, in a dog killed by the infusion of tobacco, I found the cavities of the left side of the heart to contain scarlet blood, while in those of the right side the blood was dark coloured. This poison therefore differs materially from alcohol, the essential oil of almonds, and the juice of aconite, which have no direct influence on the action of the heart. The infusion of tobacco renders the heart insensible to the stimulus of the blood, but it does not altogether destroy the power of muscular contraction, since the heart resumed its action in one instance on the division of the pericardium, and I have found that the voluntary muscles of an animal killed by this poison, are as readily stimulated to contract by the influence of the VOLTAIC battery, as if it had been killed in any other manner. At the same time, however, that the infusion of tobacco destroys the action of the heart, it appears to destroy also the functions of the brain, since these did not return in the last experiment; although the circulation was restored, and kept up by artificial respiration.

Since there is no direct communication between the intestinal canal and the heart, I was at first induced to suppose that the latter becomes affected in consequence of the infusion being conveyed into the blood by absorption. Some circumstances in the following experiment have since led me to doubt whether this is the case.

Exp. 12. In a dog, whose head was removed, I kept up the circulation by means of artificial respiration, in the manner already described in the account of some experiments, which I lately communicated to this Society. I then injected into the stomach and intestines nine ounces of infusion of tobacco, At the time of the injection, the body of the animal lay perfectly quiet and motionless on the table; the heart acted regularly one hundred times in a minute. Ten minutes afterwards the pulse rose to one hundred and forty in a minute; the peristaltic motion of the intestines was much increased, and the voluntary muscles in every part of the body were thrown into repeated and violent spasmodic action. The joints of the extremities were alternately bent and extended; the muscles of the spine, abdomen, and tail alternately relaxed and contracted, so as to turn the whole animal from one side to the other. I have observed, in other instances, spasmodic actions of the muscles, where the circulation was kept up by artificial respiration, after the removal of the head, but not at all to be compared, either in strength or frequency, with those, which took place on this occasion. I made pressure on the abdominal aorta for more than a minute, so as to obstruct the circulation of the blood in the lower extremities; but the muscular contractions were not lessened in consequence. Half an hour after the injection of the infusion, the artificial respiration was discontinued. The heart continued to act, circulating dark coloured blood; the muscular contractions continued, but gradually diminished in strength and frequency. I tied a ligature round the vessels at the base of the heart, so as to stop the circulation, nevertheless the muscular contractions still continued, though less frequent and forcible than before, and some minutes elapsed before they entirely ceased.

In this experiment, the disposition to contraction in the muscles was very much increased, instead of being diminished, as in those just related. If the infusion of tobacco influences the heart from being absorbed into the blood, and thus coming into actual contact with its fibres, there is no evident reason why, the removal of the brain, and the employment of artificial respiration, should occasion so material a difference in its effects. If the contractions of the voluntary muscles had depended on the infusion circulating with the blood, it is reasonable to suppose that the pressure on the aorta would have occasioned some diminution of them, and that the complete obstruction of the circulation would have caused them to cease altogether.

From these considerations, I am induced, on the whole, to believe that the infusion of tobacco, when injected into the intestines, influences the heart through the medium of the nervous system; but I have not been able to devise any experiment, by which the truth or fallacy of this opinion might be put beyond the reach of doubt.

It appears remarkable, that the brain and nervous system, although not necessary to the action of the heart, should, when under the influence of the infusion of tobacco, be capable of influencing this organ so as to stop its action; but this is analogous to what we see occur in consequence of violent emotions of the mind. Those states of the nervous system, which accompany the passions of joy, fear, or anger, when existing in a moderate degree, render the heart more sensible to the stimulus of the blood, and increase the frequency of its contractions; while, when the same passions exist in a greater degree, the heart is rendered altogether insensible to the stimulus of the blood, and syncope ensues.

Experiments with the Empyreumatic Oil of Tobacco.[*]

Exp. 13. Less than a drop of this oil was applied to the tongue of a young cat. Instantly violent convulsions took place in all the muscles, and the respirations became very frequent. In five minutes after the application, she lay on one side insensible, with slight spasmodic actions of the muscles. At the end of eleven minutes, she retched, but did not vomit. In a quarter of an hour, she appeared to be recovering. I repeated the application of the poison, and she was again seized with violent convulsions, and became insensible, breathing at long intervals, and in two minutes from the second application respiration had entirely ceased, and she was apparently dead. On opening the thorax, I found the heart acting with regularity and strength, circulating dark-coloured blood. I introduced a tube into the trachea, and produced artificial respiration; the contractions of the heart became augmented in force and frequency, and there was no evident diminution in six or seven minutes, during which the artificial respiration was continued.

On dissection, nothing remarkable was found in the appearance of the tongue or brain.

The symptoms and mode of death, in this experiment, did not essentially differ from those produced by the essential oil of almonds. I was surprised to find the effects of the empyreumatic oil so entirely different from those of the infusion of tobacco. Supposing that this difference might arise from the poison being more concentrated in the oil than in the infusion, I made the following experiments.

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[*] I was furnished with the empyreumatic oil of tobacco by Mr. W. Brande. It may be procured by subjecting the leaves of tobacco to distillation in a heat above that of boiling water: a quantity of watery fluid comes over, on the surface of which is a thin film of unctuous substance.

Exp. 14. A drop of the oil of tobacco was suspended in an ounce and a half of water by means of mucilage of gum arabic, and the whole was injected into the rectum of a dog. In two minutes afterwards he became faint, retched, but did not vomit. He appeared to be recovering from this state, and in twenty-five minutes after the first injection, it was repeated in the same quantity. He was then seized with symptoms similar to those in the last experiment, and in two minutes and a half he was apparently dead.

Two minutes after apparent death, on the thorax being opened into, the heart was found acting, regularly one hundred times in a minute, and it continued acting for several minutes.

Exp. 15. A drop of the empyreumatic oil of tobacco with an ounce of water was injected into the rectum of a cat. The symptoms produced were in essential circumstances similar to those, which occurred in the last experiment. The animal was apparently dead in five minutes after the injection, and the heart continued to contract for several minutes afterwards.

We may conclude from these experiments, that the empyreumatic oil of tobacco, whether applied to the tongue, or injected into the intestine, does not stop the action of the heart and induce syncope, like the infusion of tobacco; but that it occasions death by destroying the functions of the brain, without directly acting on the circulation. In other words, its effects are similar to those of alcohol, the juice of aconite, and the essential oil of almonds.

III. *Experiments with Poisons applied to wounded Surfaces.*

Experiments with the Essential Oil of Almonds.

Exp. 16. I made an incision in the thigh of a rabbit, and introduced two drops of essential oil between the skin and the muscles. In four minutes after the application, he was seized with violent convulsions, and became insensible, and in two minutes more he was apparently dead; but the heart was felt through the ribs acting one hundred and twenty times in a minute, and it continued acting for several minutes. There were no other appearances in the limb, than would have resulted from an ordinary wound.

Exp. 17. Two drops of the essential oil of almonds were introduced into a wound in the side of a mouse. Two minutes afterwards he was affected with symptoms similar to those which occurred in the last experiment, and in two minutes more he was apparently dead, but the heart continued to contract for some minutes afterwards.

From the experiments, which I have just related, and from others which it appears unnecessary to detail, as the general results were the same, I have learned that where the essential oil of almonds is applied to a wound, its effects are not so instantaneous as when it is applied to the tongue; otherwise there is no difference in its effects in whatever manner it is applied.

Experiment with the Juice of the Leaves of Aconite.

Exp. 18. I made a wound in the side of a young rabbit, and introduced, between the skin and muscles, about twenty drops of the juice of aconite. Twenty-three minutes afterwards he was affected with symptoms in all essential respects similar to those, which occurred in an experiment already related, where the juice was injected into the rectum, and at the end of forty-seven minutes from the application of the poison, he was apparently dead. Two minutes after apparent death, the heart was found contracting, but very feebly.

*Experiments with the Woorara.**

Exp. 19. A small quantity of the woorara in powder was applied to a wound in the side of a Guinea pig. Ten minutes afterwards the animal was unable to walk; then he became quite motionless, except some slight occasional convulsions. He gradually became insensible, the respirations were laboured, and at the end of fourteen minutes from the application of the poison, the respiration had entirely ceased, and he was apparently dead; but on opening the thorax, the heart was found acting seventy times in a minute, circulating dark coloured blood, and it continued to contract for several minutes afterwards. On dissection no preternatural appearances were observed in the brain; nor was there any other appearance in the limb than would have arisen from an ordinary wound.

Exp. 20. I made a wound in the side of a Guinea pig, and introduced into it about two grains of the woorara in powder.

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* The Woorara is a poison with which the Indians of Guiana arm the points of their arrows. It appears not to differ essentially from the Ticunas, which was employed in the experiments of the Abbé FONTANA. I am indebted to Dr. E. N. BANCROFT, who not only furnished me with some of the Woorara which he had in his possession, but also lent me his assistance in the experiments which were made with it.

At the end of twenty-five minutes, symptoms took place very similar to those, which occurred in the last experiment, and in thirteen minutes more the animal was apparently dead; but the heart continued to contract one hundred and eight times in a minute, and by means of artificial respiration the circulation was kept up for more than twenty minutes.

The results of other experiments, which I have made with the woorara, were similar to those just described. The heart continued to act after apparent death, and the circulation might be kept up by means of artificial respiration. It is evident that this poison acts in some way or another on the brain, and that the cessation of the functions of this organ is the immediate cause of death.

I found in these experiments, that the best mode of applying the woorara is when it is dissolved in water to the consistence of a thin paste. I first made the wound, and then smeared the poison over it with the end of the scalpel. I found that the animal was more speedily and certainly affected, if there was some hæmorrhage, unless the hæmorrhage was very copious, when it produced an opposite effect, by washing the poison away from the wound. When the poison was applied in large quantity, it sometimes began to act in six or seven minutes. Never more than half an hour elapsed from the time of the poison being inserted, to that of the animal being affected, except in one instance, where a ligature was applied on the limb, which will be mentioned afterwards. The woorara, which I employed, had been preserved for some years, which will account for its having been less active, than it has been described to be by those, who had witnessed its effects when in a recent state.

*Experiments with the Upas Antiar.**

Exp. 21. About two grains of this poison were made into a thin paste with water, and inserted into a wound in the thigh of a dog. Twelve minutes afterwards he became languid; at the end of fifteen minutes, the heart was found to beat very irregularly, and with frequent intermissions; after this, he had a slight rigor. At the end of twenty minutes, the heart beat very feebly and irregularly; he was languid; was sick and vomited; but the respirations were as frequent and as full as under natural circumstances, and he was perfectly sensible. At the end of twenty minutes, he suddenly fell on one side, and was apparently dead. I immediately opened into the thorax, and found the heart distended with blood in a very remarkable degree, and to have entirely ceased contracting. There was one distinct and full inspiration after I had begun making the incision into the thorax. The cavities of the left side of the heart contained scarlet blood, and those of the right side contained dark coloured blood, as in a living animal.

Exp. 22. A small quantity of the upas antiar, prepared as before, was inserted into a wound in the thigh of a young cat. She appeared languid in two minutes after the poison was inserted. The symptoms, which took place did not essentially differ from those which occurred in the last experiment, except that there were some convulsive motions of the limbs.

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* We are informed, that the island of Java produces two powerful vegetable poisons, to one of which the natives give the name of *Upas tieutè*, and to the other that of *Upas antiar*. I was supplied with a quantity of the latter through the kindness of Mr. Marsden, who had some of it in his possession.

At eight minutes after the poison was inserted, she lay on one side, motionless and insensible, the heart could not be felt, but the respiration had not entirely ceased. On opening into the thorax, I found the heart to have ceased contracting. It was much distended with blood: and the blood in the cavities of the left side was of a scarlet colour. There were two full inspirations after the incision of the thorax was begun. On irritating the heart with the point of the scalpel, slight contractions took place in the fibres of the appendices of the auricles, but none in any other part.

Exp. 23. The experiment was repeated on a rabbit. The symptoms produced were similar to those in the last experiment; but the animal did not vomit, and the convulsive motions were in a less degree: he died eleven minutes after the poison was inserted. On opening the chest, the heart was found to have entirely ceased contracting; it was much distended with blood; and the blood in the cavities of the left side was of a scarlet colour. On irritating the heart with the point of the scalpel, the ventricles contracted, but not sufficiently to restore the circulation.

Exp. 24. About a grain of the upas antiar was inserted into a wound in the side of rabbit. He was affected with symptoms similar to those before described, and died in ten minutes after the poison was applied. On opening the thorax immediately after death, the heart was found to have ceased contracting, and the blood in the cavities of the left side was of a scarlet colour.

It appears from these experiments, that the upas antiar, when inserted into a wound, produces death (as infusion of tobacco does when injected into the intestine) by rendering the heart insensible to the stimulus of the blood, and stopping the circulation. The heart beats feebly and irregularly before either the functions of the mind, or the respiration appear to be affected. Respiration is performed even after the circulation has ceased; and the left side of the heart is found after death to contain scarlet blood, which never can be the case, where the cause of death is the cessation of the functions of the brain or lungs. The convulsions, which occur when the circulation has nearly ceased, probably arise from the diminution of the supply of blood to the brain, resembling those, which take place in a person, who is dying from hæmorrhage.

There remains an interesting subject of inquiry, " through what medium do poisons influence the brain when applied to wounds ?" That poisons applied in this manner do not produce their effects precisely in the same way as poisons taken internally, is rendered probable by this circumstance; that, some poisons, which are very powerful when applied to wounds even in small quantities, are either altogether inefficient when taken internally, or require to be given in very large quantities, in order to produce their effect, and vice versâ.

A poison applied to a wounded surface may be supposed to act on the brain in one of three ways,

1. By means of the nerves, like poisons taken internally.

2. By passing into the circulation through the absorbent vessels.

3. By passing directly into the circulation through the divided veins.

Exp. 25. In order to ascertain whether the woorara acts through the medium of the nerves, I exposed the axilla of a rabbit, and divided the spinal nerves supplying the upper extremity, just before they unite to form the axillary plexus. The operation was performed with the greatest care. I not only divided every nervous filament, however small, which I could detect, but every portion of cellular membrane in the axilla, so that the artery and vein were left entirely insulated. I then made two wounds in the fore-arm, and inserted into them some of the woorara formed into a paste. Fourteen minutes after the poison was applied, the hind legs became paralytic, and in ten minutes more he died, with symptoms precisely similar to those, which took place in the former experiments, and the heart continued to act after apparent death. On dissection, the nerves of the upper extremity were particularly examined, but not the smallest filament could be found undivided.

I made the following experiment to ascertain whether the woorara passes into the circulation through the absorbent vessels.

Exp. 26. I tied a ligature round the thoracic duct of a dog, just before it perforates the angle of the left subclavian and jugular veins. I then made two wounds in the left hind leg, and introduced some of the woorara in powder into them. In less than a quarter of an hour he became affected with the usual symptoms, and died in a few minutes afterwards.

After death, I dissected the thoracic duct with great care. I found it to have been perfectly secured by the ligature. It was very much distended with chyle, and about two inches below its termination its coats had given way, and chyle was extravasated into the cellular membrane. The lymphatic vessels in the left axilla were distended in a very remarkable degree, and on dividing them, not less than a dram of lymph issued from the divided ends.

Since neither the division of the nerves, nor the obstruction of the thoracic duct interfere in the slightest degree with the effects of the woorara, there is presumptive evidence that it acts on the brain by entering the circulation through the divided veins. I endeavoured to ascertain, by experiment, whether this is really the case.

To apply ligatures to the large vessels of a limb only would evidently lead to no satisfactory conclusion, since the anastomosing vessels might still carry on the circulation. The only way, which I could devise of performing the experiment, was to include all the vessels, small as well as large, in a ligature.

Exp. 27. In order to make the experiment more satisfactorily, I exposed the sciatic nerve of a rabbit in the upper and posterior part of the thigh, and passed under it a tape half an inch wide. I then made a wound in the leg, and having introduced into it some of the woorara mixed with water, I tied the tape moderately tight on the fore-part of the thigh. Thus I interrupted the communication between the wounds and the other parts of the body, by means of the vessels, while that by means of the nerve still remained. After the ligature was tightened, I applied the woorara a second time, in another part of the leg. The rabbit was not at all affected, and at the end of an hour I removed the ligature. Being engaged in some other pursuit, I did not watch the animal so closely as I should otherwise have done; but twenty minutes after the ligature was removed, I found him lying on one side, motionless and insensible, evidently under the influence of the poison, but the symptoms were less violent than in most instances, and after lying in this state he recovered, and the limb became perfectly warm, and he regained the power of using it.

Exp. 28. I repeated the last experiment with this difference, that after having applied the poison, I made the ligature as tight as I could draw it. I removed the ligature at the end of an hour and twenty minutes, but the animal was not at all affected either before or after the removal of the ligature, and on the following day he had recovered the use of the limb.

Exp. 29. I repeated the experiment a third time, drawing the ligature very tight. At the end of forty-five minutes, the animal continued perfectly well, and the ligature was removed. I watched him for three quarters of an hour afterwards, but there were no symptoms of his being affected by the poison. On the following day the rabbit died, but this I attribute to the injury done to the limb and sciatic nerve by the ligature, as there was the appearance of inflammation in the parts in the neighbourhood of the ligature.

These three experiments were made with the greatest care. From the mode, in which the poison was applied, from the quantity employed, and from my prior experience, I should have entertained not the smallest doubt of the poison taking effect in every instance in less than twenty minutes, if no ligature had been applied. In two of the three, the quantity of woorara was more than had been used in any former experiments.

I have not judged it necessary to make any more experiments, with the ligature on the limb, because the numerous experiments of the Abbé Fontana on the ticunas, coincide in their results, with those, which have just been detailed, and fully establish the efficacy of the ligature, in preventing the action of the poison. It is not to be wondered at, that the ligature should sometimes fail in its effects, since these must evidently depend on the degree, in which the circulation is obstructed, and on the length of time during which the obstruction is continued.

There can be little doubt that the woorara affects the brain, by passing into the circulation through the divided vessels. It is probable that it does not produce its effects, until it enters the substance of the brain, along with the blood, in which it is dissolved; nor will the experiments of the Abbé FONTANA, in which he found the ticunas produce almost instant death when injected into the jugular vein of a rabbit, be found to militate against this conclusion, when we consider how short is the distance, which, in so small an animal, the blood has to pass from the jugular vein to the carotid artery, and the great rapidity of the circulation; since in a rabbit under the influence of terror, during such an experiment, the heart cannot be supposed to act so seldom as three times in a second.

I have made no experiments to ascertain through what medium other poisons when applied to wounds affect the vital organs, but from analogy we may suppose that they enter the circulation through the divided blood-vessels.

IV.

The facts already related led me to conclude that alcohol, the essential oil of almonds, the juice of aconite, the oil of tobacco, and the woorara, occasion death simply by destroying the functions of the brain. The following experiment appears fully to establish the truth of this conclusion.

Exp. 30. The temperature of the room being 58° of FAHRENHEIT's thermometer, I made two wounds in the side of a rabbit, and applied to them some of the woorara in the form of paste. In seven minutes after the application, the hind legs were paralysed, and in fifteen minutes respiration had ceased, and he was apparently dead. Two minutes afterwards the heart was still beating, and a tube was introduced through an opening into the trachea, by means of which the lungs were inflated. The artificial respiration was made regularly about thirty-six times in a minute.

At first, the heart contracted one hundred times in a minute.

At the end of forty minutes, the pulse had risen to one hundred and twenty in a minute.

At the end of an hour, it had risen to one hundred and forty in a minute.

At the end of an hour and twenty-three minutes, the pulse had fallen to a hundred, and the artificial respiration was discontinued.

At the commencement of the experiment, the ball of a thermometer being placed in the rectum, the quicksilver rose to one hundred degrees; at the close of the experiment it had fallen to eighty-eight and a half.

During the continuance of the artificial respiration, the blood in the femoral artery was of a florid red, and that in the femoral vein of a dark colour, as usual.

It has been observed by M. Bichat, that the immediate cause of death, when it takes place suddenly, must be the cessation of the functions of the heart, the brain, or the lungs. This observation may be extended to death under all circumstances. The stomach, the liver, the kidneys, and many other organs are necessary to life, but their constant action is not necessary; and the cessation of their functions cannot therefore be the *immediate* cause of death. As in this case the action of the heart had never ceased; as the circulation of the blood was kept up by artificial respiration for more than an hour and twenty minutes after the poison had produced its full effects; and as during this time the usual changes in the colour of the blood took place in the lungs; it is evident that the functions of the heart and lungs were unimpaired: but that those of the brain had ceased, is proved, by the animal having continued in a state of complete insensibility, and by this circumstance, that animal heat, to the generation of which I have formerly shewn the influence of the brain to be necessary, was not generated.

Having learned that the circulation might be kept up by artificial respiration for a considerable time after the woorara had produced its full effects, it occurred to me that in an animal under the influence of this or of any other poison that acts in a similar manner, by continuing the artificial respiration for a sufficient length of time after natural respiration had ceased, the brain might recover from the impression, which the poison had produced, and the animal might be restored to life. In the last experiment, the animal gave no sign of returning sensibility; but it is to be observed, 1. That the quantity of the poison employed was very large. 2. That there was a great loss of animal heat, in consequence of the temperature of the room being much below the natural temperature of the animal, which could not therefore be considered under such favourable circumstances as to recovery, as if it had been kept in a higher temperature. 3. That the circulation was still vigorous when I left off inflating the lungs, and therefore it cannot be known what would have been the result, if the artificial respiration had been longer continued.

Exp. 30. A wound was made in the side of a rabbit, and one drop of the essential oil of almonds was inserted into it, and immediately the animal was placed in a temperature of 90°. In two minutes he was under the influence of the poison. The usual symptoms took place, and in three minutes more respiration had ceased, and he lay apparently dead, but the heart was still felt beating through the ribs. A tube was then introduced into one of the nostrils, and the lungs were inflated about thirty-five times in a minute. Six minutes after the commencement of artificial respiration, he moved his head and legs, and made an effort to breathe. He then was seized with convulsions, and again lay motionless, but continued to make occasional efforts to breathe. Sixteen minutes after its commencement, the artificial respiration was discontinued. He now breathed spontaneously seventy times in a minute, and moved his head and extremities. After this, he occasionally rose, and attempted to walk. In the intervals, he continued in a dozing state; but from this he gradually recovered. In less than two hours he appeared perfectly well, and he continued well on the following day.

The inflating the lungs has been frequently recommended in cases of suffocation, where the cause of death is the cessation of the functions of the lungs: as far as I know, it has not been before proposed in those cases, in which the cause of death is the cessation of the functions of the brain.* It is probable that this method of treatment might be employed with advantage for the recovery of persons labouring under the effects of opium, and many other poisons.

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* Since this paper was read, I have been favoured by the Right Hon. the President with the perusal of a Dissertation on the Effects of the Upas Tieutè, lately published

V.

The experiments, which have been detailed lead to the following conclusions.

1. Alcohol, the essential oil of almonds, the juice of aconite, the empyreumatic oil of tobacco, and the woorara, act as poisons by simply destroying the functions of the brain; universal death taking place, because respiration is under the influence of the brain, and ceases when its functions are destroyed.

2. The infusion of tobacco when injected into the intestine, and the upas antiar when applied to a wound, have the power of rendering the heart insensible to the stimulus of the blood, thus stopping the circulation; in other words, they occasion syncope.

3. There is reason to believe that the poisons, which in these experiments were applied internally, produce their effects through the medium of the nerves without being absorbed into the circulation.

4. When the woorara is applied to a wound, it produces its effects on the brain, by entering the circulation through the divided blood-vessels, and, from analogy, we may conclude that other poisons, when applied to wounds, operate in a similar manner.

5. When an animal is apparently dead from the influence of a poison, which acts by simply destroying the functions of

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at Paris by M. DELILE, by which I find that he had employed artificial respiration for the purpose of recovering animals, which were under the influence of this poison, with success. M. DELILE describes the Upas Tieutè as causing death, by occasioning repeated and long continued contractions of the muscles of respiration, on which it acts through the medium of the spinal marrow, without destroying the functions of the brain.

the brain, it may, in some instances, at least, be made to recover, if respiration is artificially produced, and continued for a certain length of time.

From analogy we might draw some conclusions respecting the mode in which some other vegetable poisons produce their effects on the animal system; but I forbear to enter into any speculative inquiries; as it is my wish, in the present communication, to record such facts only, as appear to be established by actual experiment.

Addition to the Croonian Lecture for the Year 1810.

In the experiments formerly detailed, where the circulation was maintained by means of artificial respiration after the head was removed, I observed that the blood, in its passage through the lungs, was altered from a dark to a scarlet colour, and hence I was led to conclude that the action of the air produced in it changes analogous to those, which occur under ordinary circumstances. I have lately, with the assistance of my friend Mr. W. Brande, made the following experiment, which appears to confirm the truth of this conclusion.

An elastic gum bottle, having a tube and stop-cock connected with it, was filled with about a pint of oxygen gas. The spinal marrow was divided in the neck of a young rabbit, and the blood-vessels having been secured, the head was removed, and the circulation was maintained by inflating the lungs with atmospheric air for five minutes, at the end of which time the tube of the gum bottle was inserted into the trachea, and carefully secured by a ligature, so that no air might escape. By making pressure on the gum bottle, the gas was made to pass and repass into and from the lungs about thirty times in a minute. At first, the heart acted one hundred and twenty times in a minute, with regularity and strength; the thermometer, in the rectum, rose to 100°. At the end of an hour, the heart acted as frequently as before, but more feebly; the blood in the arteries was very little more florid than that in the veins; the thermometer in the rectum had fallen to 93°. The gum bottle was then removed. On causing a stream of the gas, which it contained, to pass through lime-water, the presence of carbonic acid was indicated by the liquid being instantly rendered turbid. The proportion of carbonic acid was not accurately determined; but it appeared to form about one-half of the quantity of gas in the bottle.

B. C. BRODIE.

ERRATA.

Page 39, line 13, *for* artery *read* ureter.
47, last line of table 4th col. *for* 9½ *read* 91½.

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METEOROLOGICAL JOURNAL,

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ROYAL SOCIETY,

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PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
		H.	M.	°	°	Inches.			Points	Str.	
	°						°				
Jan. 1	46	8	0	48	52	30,10	75		W	2	Cloudy.
	51	2	0	51	56	30,14	72		SW	2	Cloudy.
2	46	8	0	46	52	30,22	75		S	2	Cloudy.
	47	2	0	47	56	30,20	72		S	2	Cloudy.
3	44	8	0	46	53	30,15	73		WSW	1	Cloudy.
	50	2	0	50	56	30,18	73		S	1	Cloudy.
4	45	8	0	45	55	30,25	74		S	1	Cloudy.
	50	2	0	50	57	30,30	73		S	1	Cloudy.
5	47	8	0	47	55	30,34	70		S	1	Cloudy.
	47	2	0	44	57	30,31	63		E	1	Cloudy.
6	42	8	0	42	54	30,30	70		SSW	1	Cloudy.
	49	2	0	49	57	30,31	70		SSW	1	Cloudy.
7	43	8	0	43	54	30,24	70		S	1	Cloudy.
	45	2	0	45	55	30,16	68		S	1	Cloudy.
8	39	8	0	39	53	30,03	68		E	1	Cloudy.
	45	2	0	41	54	29,93	70		ESE	1	Cloudy.
9	41	8	0	46	53	29,84	75		ESE	1	Rain.
	48	2	0	48	55	29,80	73		SE	2	Cloudy.
10	40	8	0	41	52	29,97	70		E	1	Cloudy.
	45	2	0	44	55	29,98	69		ESE	1	Cloudy.
11	41	8	0	44	53	29,91	73		E	1	Cloudy.
	47	2	0	46	55	29,88	68		SE	1	Cloudy.
12	44	8	0	44	53	29,87	70		E	1	Cloudy.
	45	2	0	44	55	29,87	68		E	1	Cloudy.
13	34	8	0	34	51	29,93	65		E	2	Cloudy.
	34	2	0	33	54	29,96	62		E	2	Cloudy.
14	27	8	0	27	48	30,04	60		E	2	Cloudy.
	30	2	0	29	52	29,98	60		E	2	Fair.
15	25	8	0	25	47	29,86	67		NE	2	Snow.
	29	2	0	28	50	29,82	62		NE	2	Fair.
16	18	8	0	20	45	29,75	68		SW	1	Cloudy.
	26	2	0	26	48	29,73	68		SSW	1	Cloudy.

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1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
		H.	M.	°	°	Inches.			Points.	Str.	
Jan. 17	14°	8	0	18	43	30,00	68°		WSW	1	Cloudy.
	31	2	0	31	46	30,04	68		SW	1	Fair.
18	25	8	0	28	43	30,23	73		W	1	Foggy.
	37	2	0	37	47	30,24	71		NE	1	Snow.
19	23	8	0	23	43	30,24	73		NE	1	Foggy.
	33	2	0	33	47	30,22	70		ENE	1	Foggy.
20	18	8	0	20	42	30,17	72		NE	1	Foggy.
	26	2	0	26	46	30,13	70		NE	1	Foggy.
21	24	8	0	28	42	30,02	74		NE	1	Cloudy.
	36	2	0	36	46	29,99	70		NE	1	Cloudy.
22	29	8	0	30	42	29,94	72		NE	1	Cloudy.
	35	2	0	35	45	29,91	68		NE	1	Cloudy.
23	33	8	0	34	43	30,08	75		NE	1	Foggy.
	38	2	0	38	46	30,13	75		NNE	1	Cloudy.
24	35	8	0	35	43	30,18	75		NE	1	Cloudy.
	38	2	0	38	47	30,20	73		NE	1	Cloudy.
25	33	8	0	33	43	30,27	72		ESE	1	Cloudy.
	35	2	0	35	47	30,30	72		SSW	1	Cloudy.
26	31	8	0	31	45	30,33	73		NE	1	Cloudy.
	37	2	0	37	47	30,28	73		NE	1	Cloudy.
27	32	8	0	32	46	30,26	73		E	1	Cloudy.
	32	2	0	32	48	30,23	73		SE	1	Cloudy.
28	30	8	0	30	44	30,22	72		N	1	Cloudy.
	31	2	0	31	47	30,21	72		N	1	Cloudy.
29	30	8	0	31	44	30,28	72		N	1	Cloudy.
	34	2	0	34	46	30,32	72		E	1	Cloudy.
30	31	8	0	32	44	30,44	72		E	1	Cloudy.
	33	2	0	33	46	30,44	72		E	1	Cloudy.
31	30	8	0	31	43	30,36	74		SE	1	[illegible]
	45	2	0	43	47	30,25	75		S	1	[illegible]

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for February, 1810.

1810	Six's Therm least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Winds.		Weather.
	°	H.	M.	°	°	Inches.	°	Points.	Str.	
Feb. 1	43	7	0	46	46	30,16	78	SSW	1	Cloudy.
	50	2	0	50	50	30,11	78	SW	1	Cloudy.
2	46	7	0	46	49	30,07	78	SSW	1	Cloudy.
	47	2	0	47	52	30,02	78	SW	1	Rain.
3	43	7	0	43	50	29,79	78	S	1	Foggy.
	47	2	0	46	53	29,68	78	S	1	Cloudy.
4	43	7	0	43	51	29,88	76	N	1	Cloudy.
	45	2	0	45	53	30,02	67	NE	1	Cloudy.
5	35	7	0	35	50	30,08	72	N	1	Fair.
	46	2	0	46	53	30,04	71	S	1	Cloudy.
6	39	7	0	39	50	30,03	74	SSW	1	Fair.
	47	2	0	47	53	30,00	70	SW	1	Cloudy.
7	45	7	0	45	52	29,94	77	SSW	1	Cloudy.
	49	2	0	49	53	29,92	74	SSW	1	Cloudy.
8	45	7	0	45	52	29,95	76	S	1	Cloudy.
	48	2	0	48	54	29,95	74	S	2	Cloudy.
9	43	7	0	43	52	29,79	73	S	2	Cloudy.
	48	2	0	47	54	29,73	74	S	2	Cloudy.
10	47	7	0	47	53	29,76	76	SSW	1	Cloudy.
	51	2	0	51	56	29,80	66	N	1	Cloudy.
11	43	7	0	43	54	29 87	69	E	1	Cloudy.
	43	2	0	43	56	29,78	69	E	2	Cloudy.
12	40	7	0	40	53	29,41	76	E	1	Rain.
	42	2	0	42	54	29,31	76	E	1	Rain.
13	38	7	0	38	53	29,02	72	W	1	Cloudy.
	45	2	0	45	55	29,01	60	W	2	Fair.
14	36	7	0	36	52	29,32	70	NW	1	Cloudy.
	40	2	0	40	53	29,38	66	N	1	Cloudy.
15	36	7	0	36	52	29,69	63	NE	2	Cloudy.
	41	2	0	41	53	29,80	61	NE	2	Cloudy.
16	28	7	0	28	51	30 08	64	N	1	Cloudy.
	38	2	0	38	53	30,05	60	NW	1	Fair.

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1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.		Winds.		Weather.
	°	H.	M.	°	°	Inches.	°		Points.	Str.	
Feb. 17	27	7	0	27	50	30,12	70		NNE	1	Cloudy.
	36	2	0	36	53	30,17	66		NNE	1	Fair.
18	24	7	0	24	48	30,18	65		NW	1	Cloudy.
	35	2	0	35	50	30,09	63		SW	1	Snow.
19	29	7	0	29	47	29,98	65		N	1	Cloudy.
	35	2	0	35	50	30,04	60		N	2	Fine.
20	27	7	0	27	46	30,20	69		NNE	1	Snow.
	32	2	0	32	49	30,28	61		NE	1	Fair.
21	18	7	0	19	43	30,44	66		NE	1	Cloudy.
	31	2	0	30	47	30,44	62		W	1	Cloudy.
22	25	7	0	26	43	30,28	63		W	1	Cloudy.
	36	2	0	36	46	30,18	60		W	2	Cloudy.
23	30	7	0	34	44	29,54	73		SW	2	Rain.
	46	2	0	45	47	29,39	75		SW	2	Cloudy.
24	40	7	0	41	46	29,54	76		W	1	Cloudy.
	51	2	0	50	49	29,55	75		WSW	2	Cloudy.
25	47	7	0	48	49	29,47	67		WSW	1	Cloudy.
	51	2	0	49	53	29,50	58		NW	2	Cloudy.
26	37	7	0	38	48	29,87	63		W	1	Cloudy.
	47	2	0	47	52	29,93	57		W	1	Fair.
27	45	7	0	48	51	29,72	75		WSW	1	Cloudy.
	54	2	0	54	53	29,66	68		SW	2	Cloudy.
28	42	7	0	43	52	30,01	65		SW	1	Cloudy.
	54	2	0	53	56	30,03	61		SW	1	Fair.

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for March, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.		Winds.		Weather.
	°	H.	M.	°	°	Inches.	°		Points.	Str.	
Mar. 1	48	7	0	48	53	29,89	75		SW	2	Cloudy.
	55	2	0	54	56	29,80	67		SW	2	Cloudy.
2	49	7	0	50	55	29,70	76		SW	1	Cloudy.
	54	2	0	53	57	29,70	73		SW	1	Cloudy.
3	48	7	0	49	56	29,61	76		SSW	1	Cloudy.
	54	2	0	53	58	29,56	74		S	1	Rain.
4	40	7	0	40	55	29,70	76		SW	1	Cloudy.
	50	2	0	49	56	29,61	70		S	1	Cloudy.
5	42	7	0	42	55	29,28	72		WNW	1	Cloudy.
	47	2	0	47	56	29,17	63		N	1	Fair.
6	36	7	0	36	54	28,93	75		NE	1	Snow.
	43	2	0	40	56	28,95	72		NE	1	Cloudy.
7	40	7	0	42	54	28,92	73		E	1	Cloudy.
	45	2	0	45	56	28,84	73		E	2	Rain.
8	40	7	0	44	53	28,95	73		E	2	Cloudy.
	52	2	0	51	56	29,00	67		S	2	Cloudy.
9	47	7	0	50	56	29,12	75		S	2	Cloudy.
	58	2	0	56	57	29,25	68		S	2	Rain.
10	50	7	0	52	56	29,42	65		SW	1	Cloudy.
	59	2	0	58	59	29,58	57		W	2	Fair.
11	47	7	0	49	57	29,92	73		SW	1	Cloudy.
	58	2	0	57	60	29,94	63		SW	1	Cloudy.
12	51	7	0	52	58	29,70	70		W	1	Cloudy.
	57	2	0	56	61	29,68	65		WSW	1	Cloudy.
13	41	7	0	41	57	30,01	64		E	1	Cloudy.
	45	2	0	45	58	30,04	63		E	1	Cloudy.
14	40	7	0	40	56	30,02	61		NE	1	Cloudy.
	44	2	0	43	58	30,02	58		NE	1	Cloudy.
15	35	7	0	36	54	29,98	59		E	2	Cloudy.
	41	2	0	40	56	29,73	56		E	2	Cloudy.
16	33	7	0	34	53	29,55	62		NE	2	Cloudy.
	41	2	0	40	50	29,60	60		N	1	Cloudy.

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1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
	°	H.	M.	°	°	Inches.	°		Points.	Str.	
Mar. 17	34	7	0	36	53	29,67	62		NNW	2	Fair.
	43	2	0	42	56	29,70	53		N	2	Fair.
18	30	7	0	34	52	29,89	60		NNE	2	Fair.
	40	2	0	40	56	29,91	55		NE	2	Fair.
19	28	7	0	30	51	30,01	63		W	1	Fair.
	46	2	0	44	56	30,05	55		W	1	Fair.
20	34	7	0	36	52	30,05	66		NW	1	Hazy.
	50	2	0	49	55	30,00	60		NNE	1	Hazy.
21	33	7	0	38	50	29,76	65		WNW	1	Cloudy.
	50	2	0	49	55	29,74	49		NNW	1	Cloudy.
22	33	7	0	37	50	29,93	65		N	1	Fair.
	44	2	0	43	56	30,03	49		NNE	1	Hazy.
23	30	7	0	36	52	29,98	61		NE	1	Hazy.
	49	2	0	49	57	29,86	56		W	1	Hazy.
24	30	7	0	35	52	29,76	64		E	1	Fair.
	47	2	0	46	56	29,76	58		E	2	Hazy.
25	36	7	0	37	51	29,78	60		E	2	Hazy.
	42	2	0	41	54	29,81	56		E	2	Cloudy.
26	34	7	0	36	50	29,93	60		E	2	Hazy.
	43	2	0	42	54	29,92	57		E	2	Hazy.
27	38	7	0	43	51	29,77	66		E	1	Cloudy.
	56	2	0	51	55	29,66	67		ESE	1	Cloudy.
28	43	7	0	44	52	29,65	67		NW	2	Fair.
	52	2	0	52	55	29,73	68		WNW	2	Fair.
29	39	7	0	43	52	29,91	68		WNW	1	Cloudy.
	54	2	0	53	56	29,92	55		WNW	1	Cloudy.
30	39	7	0	40	54	29,94	65		WNW	1	Cloudy.
	51	2	0	50	55	29,88	57		SW	1	Cloudy.
31	44	7	0	46	54	29,77	69		S	1	Cloudy.
	52	2	0	52	56	29,68	62		SSE	1	Cloudy.

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for April, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
	°	H.	M.	°	°	Inches.	°		Points.	Str.	
Apr. 1	43	7	0	44	53	29,43	70		ESE	1	Rain.
	51	2	0	50	55	29,43	64		E	2	Cloudy.
2	44	7	0	44	53	29,70	71		E	1	Cloudy.
	48	2	0	48	55	29,76	65		E	1	Cloudy.
3	44	7	0	46	54	29,63	73		S	1	Cloudy.
	57	2	0	56	58	29,56	69		S	2	Cloudy.
4	43	7	0	44	54	29,43	70		W	2	Rain.
	50	2	0	49	55	29,57	63		WNW	2	Cloudy.
5	37	7	0	41	53	29,76	67		W	2	Cloudy.
	52	2	0	50	57	29,69	53		WSW	2	Cloudy.
6	44	7	0	44	54	29,36	65		E	2	Cloudy.
	48	2	0	47	55	29,29	63		E	2	Cloudy.
7	43	7	0	46	54	29,32	68		E	2	Cloudy.
	54	2	0	54	57	29,36	60		SE	2	Cloudy.
8	42	7	0	45	54	29,48	70		E	2	Cloudy.
	54	2	0	53	56	29,48	57		E	2	Cloudy.
9	44	7	0	47	53	29,46	74		E	2	Cloudy.
	54	2	0	53	56	29,44	63		E	2	Cloudy.
10	44	7	0	44	54	29,52	75		NE	1	Rain.
	45	2	0	45	56	29,57	76		N	2	Rain.
11	36	7	0	38	52	29,66	68		NNE	2	Cloudy.
	45	2	0	43	54	29,68	65		N	2	Cloudy.
12	34	7	0	36	51	29,91	64		NE	2	Cloudy.
	43	2	0	43	53	29,93	54		NNE	2	Cloudy.
13	31	7	0	35	51	29,94	64		N	2	Cloudy.
	44	2	0	43	53	29,93	55		NNE	2	Cloudy.
14	35	7	0	38	51	29,90	65		N	2	Cloudy.
	46	2	0	45	52	29,88	54		WNW	1	Cloudy.
15	40	7	0	44	51	29,82	66		SSW	1	Cloudy.
	52	2	0	50	53	29,79	56		SSW	2	Fair.
16	38	7	0	42	51	29,58	62		E	2	Hazy.
	51	2	0	49	51	29,45	54		E	2	Cloudy.

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1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
		H.	M.	°	°	Inches.			Points	Str.	
	°						°				
Apr. 17	39	7	0	42	50	29,46	68		W	2	Fair.
	54	2	0	52	54	29,54	59		SSW	2	Fair.
18	39	7	0	47	52	29,63	71		SSE	2	Hazy.
	59	2	0	57	56	29,62	54		SE	2	Fair.
19	46	7	0	49	53	29,74	68		SSW	2	Fair.
	60	2	0	59	56	29,78	66		ESE	2	Fair.
20	45	7	0	48	54	29,93	65		SW	2	Cloudy.
	61	2	0	60	56	30,07	60		SW	2	Cloudy.
21	46	7	0	49	54	30,17	67		W	1	Fair.
	62	2	0	62	58	30,19	55		W	2	Fair
22	44	7	0	48	55	30,20	67		W	1	Cloudy.
	63	2	0	63	57	30,18	54		WNW	[illegible]	Fine.
23	48	7	0	53	56	30,18	65		W	1	Hazy.
	66	2	0	66	59	30,19	57		E	1	Fair.
24	47	7	0	48	57	30,21	67		E	1	Hazy.
	63	2	0	63	59	30,19	58		E	1	Fair.
25	44	7	0	49	57	30,17	62		NE	1	Hazy.
	58	2	0	58	59	30,14	63		ENE	2	Fair.
26	44	7	0	49	56	30,12	63		E	1	Hazy.
	57	2	0	57	58	30,12	60		E	1	Fair.
27	46	7	0	50	56	30,10	59		E	1	Hazy.
	57	2	0	57	59	30,09	49		E	1	Fine.
28	45	7	0	49	56	30,09	60		E	1	Hazy.
	65	2	0	64	60	30,10	49		E	1	Fine.
29	44	7	0	49	58	30,10	59		NNE	1	Hazy.
	69	2	0	69	62	30,04	46		E	1	Fine.
30	48	7	0	53	59	29,99	55		NNE	1	Hazy.
	69	2	0	69	61	29,94	49		E	2	Fine.

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for May, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
		H.	M.	°	°	Inches.			Points.	Str.	
	°						°				
May 1	48	7	0	51	59	29,86	56		NNE	1	Hazy.
	65	2	0	64	61	29,84	51		NE	1	Fine.
2	44	7	0	46	59	29.87	56		NNE	1	Hazy.
	56	2	0	55	59	29.84	54		NNE	1	Cloudy.
3	44	7	0	46	58	29 75	63		NW	1	Rain.
	52	2	0	52	59	29,73	60		N	2	Cloudy.
4	40	7	0	45	57	29,72	63		W	1	Cloudy.
	55	2	0	55	57	29,75	57		NNE	1	Cloudy.
5	39	7	0	43	55	29,85	56		NE	1	Cloudy.
	50	2	0	49	55	29,84	52		E	1	Cloudy.
6	34	7	0	40	54	29 81	58		NNE	1	Fair.
	51	2	0	50	55	29,80	49		N	1	Cloudy.
7	40	7	0	44	53	29,70	57		NE	2	Cloudy.
	52	2	0	48	54	29.56	69		ENE	1	Cloudy.
8	48	7	0	52	54	29.41	75		SW	1	Cloudy.
	63	2	0	62	57	29,55	48		WNW	1	Cloudy.
9	45	7	0	50	55	29,77	65		SW	1	Hazy.
	62	2	0	62	57	29 78	53		SW	2	Cloudy.
10	48	7	0	52	56	29.94	59		WSW	1	Cloudy.
	65	2	0	62	60	29.96	57		N	1	Cloudy.
11	47	7	0	49	57	30.19	59		ENE	1	Cloudy.
	57	2	0	56	59	30,07	54		E	1	Fair.
12	43	7	0	46	57	30,00	60		E	1	Hazy.
	60	2	0	59	59	29.96	57		E	2	Cloudy.
13	49	7	0	51	57	29,86	64		E	2	Hazy.
	53	2	0	52	58	29,82	62		E	2	Cloudy.
14	50	7	0	52	57	29,67	61		E	2	Hazy.
	60	2	0	59	59	29,60	55		E	2	Fair.
15	47	7	0	50	57	29,39	73		N	1	Rain.
	57	2	0	56	58	29,35	68		WNW	1	Cloudy.
16	48	7	0	49	56	29,39	68		WNW	1	Rain.
	63	2	0	62	58	29,43	54		WSW	1	Cloudy.

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for May, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
	°	H.	M.	°	°	Inches.	°		Points.	Str.	
May 17	47	7	0	50	57	29,61	56		N	1	Cloudy.
	59	2	0	59	56	29,50	64		SE	1	Rain.
18	43	7	0	43	56	29,55	71		N	1	Rain.
	50	2	0	50	56	29,63	65		N	1	Rain.
19	41	7	0	43	55	29,92	66		W	1	Hazy.
	60	2	0	59	57	29,97	52		W	1	Cloudy.
20	41	7	0	50	55	29,99	62		E	1	Cloudy.
	61	2	0	60	58	29,87	53		E	1	Cloudy.
21	48	7	0	52	57	29,65	62		SW	2	Cloudy.
	63	2	0	62	59	29,62	54		SW	2	Cloudy.
22	48	7	0	52	57	29,84	64		W	1	Cloudy.
	64	2	0	62	58	29,89	54		W	1	Cloudy.
23	48	7	0	51	57	30,09	56		N	1	Cloudy.
	62	2	0	60	59	30,10	49		NNE	1	Cloudy.
24	45	7	0	50	57	30,18	58		W	1	Hazy.
	64	2	0	63	60	30,14	48		W	1	Cloudy.
25	49	7	0	51	58	30,06	62		N	1	Cloudy.
	64	2	0	64	59	30,08	57		N	1	Cloudy.
26	44	7	0	48	57	30,04	65		E	1	Hazy.
	65	2	0	63	60	29,97	49		E	1	Cloudy.
27	46	7	0	52	59	29,93	59		E	1	Cloudy.
	67	2	0	67	60	29,90	50		E	2	Cloudy.
28	48	7	0	52	59	30,04	57		NE	2	Cloudy.
	60	2	0	60	60	30,23	50		NE	2	Cloudy.
29	43	7	0	49	58	30,38	57		ENE	1	Hazy.
	60	2	0	59	60	30,38	50		E	2	Fine.
30	43	7	0	50	58	30,36	61		NNE	1	Hazy.
	64	2	0	64	60	30,34	50		ESE	2	Fine.
31	47	7	0	52	58	30,32	56		E	1	Hazy.
	66	2	0	66	61	30,31	51		E	1	Fine.

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for June, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.		Winds.		Weather.
		H.	M.	o	o	Inches.			Points.	Str.	
June 1	o 49	7	o	55	59	30,28	o 61		E	1	Hazy.
	66	2	o	66	61	30,29	48		E	2	Fine.
2	47	7	o	52	59	30,24	61		E	1	Hazy.
	69	2	o	69	62	30,22	51		E	2	Fair.
3	49	7	o	54	60	30,22	59		NNE	1	Fair.
	67	2	o	67	61	30,23	53		NE	2	Cloudy.
4	46	7	o	50	59	30,26	61		ENE	1	Fair.
	68	2	o	68	62	30,19	48		E	1	Fine.
5	53	7	o	55	60	30,23	60		NNE	2	Cloudy.
	60	2	o	60	59	30,25	56		NNE	2	Cloudy.
6	49	7	o	53	59	30,23	57		N	1	Fair.
	71	2	o	71	61	30,17	52		ENE	2	Fair.
7	55	7	o	58	61	30,14	63		E	1	Cloudy.
	72	2	o	72	62	30,11	56		N	1	Fair
8	54	7	o	60	61	30,09	61		WSW	1	Cloudy.
	74	2	o	74	63	30,04	53		SW	1	Fair.
9	58	7	o	59	62	29,96	59		N	1	Cloudy.
	73	2	o	73	64	29,92	54		E	1	Fair.
10	58	7	o	62	62	29,73	63		E	1	Cloudy.
	72	2	o	72	64	29,68	58		NNE	1	Cloudy.
11	51	7	o	56	62	29,80	58		NW	1	Fair.
	70	2	o	70	64	29,83	50		NW	2	Cloudy.
12	54	7	o	60	62	29 94	55		WNW	2	Cloudy.
	66	2	o	66	62	29,94	51		WNW	2	Cloudy.
13	53	7	o	60	61	29,89	57		SW	2	Cloudy.
	69	2	o	69	62	29,91	52		SW	2	Rain.
14	46	7	o	52	60	30 08	59		NW	2	Fair.
	65	2	o	65	61	30,15	49		NW	2	Fair.
15	47	7	o	53	60	30,24	58		WNW	1	Cloudy.
	69	2	o	69	61	30,14	53		NW	2	Fair.
16	48	7	o	53	60	30,08	60		N	2	Cloudy.
	65	2	o	65	62	29,99	51		NE	2	Cloudy.

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for June, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
	°	H.	M.	°	°	Inches.	°		Points.	Str.	
June 17	45	7	0	53	60	29,97	56		NE	2	Cloudy.
	66	2	0	66	61	29,92	52		NW	2	Fair.
18	50	7	0	56	60	29,93	55		WNW	1	Fair.
	75	2	0	75	62	29,93	48		WNW	1	Fine.
19	58	7	0	63	62	29,96	60		WNW	2	Cloudy.
	70	2	0	70	63	29.95	56		WSW	1	Cloudy.
20	59	7	0	62	63	29 94	64		WNW	2	Cloudy.
	73	2	0	73	64	29.96	60		WNW	2	Cloudy.
21	56	7	0	6[illegible]	62	30,16	60		WNW	1	Fair.
	78	2	0	78	66	30,18	48		NW	1	Fair.
22	62	7	0	65	65	30,28	61		N	1	Cloudy.
	75	2	0	75	66	30,29	56		ENE	1	Fair.
23	53	7	0	60	60	30.38	63		E	1	Fair.
	70	2	0	70	65	30,32	58		ENE	1	Fair.
24	50	7	0	58	60	30.24	63		E	1	Cloudy.
	72	2	0	72	65	30,17	55		E	1	Fine.
25	55	7	0	59	62	30,13	63		E	1	Cloudy.
	77	2	0	77	66	30,10	61		E	1	Fine.
26	59	7	0	60	63	3[illegible],06	59		NNE	2	Cloudy.
	64	2	0	62	63	30,05	59		NNE	1	Cloudy.
27	53	7	0	56	63	30 01	58		NNW	1	Fair.
	68	2	0	68	63	29 94	52		N	1	Cloudy.
28	52	7	0	57	59	29.96	61		E	1	Cloudy.
	71	2	0	71	64	29.98	55		E	1	Fair.
29	57	7	0	61	62	29.96	64		E	1	Cloudy.
	74	2	0	74	65	29,98	56		E	1	Cloudy.
30	54	7	0	59	63	30,10	64		W	1	Fair.
	71	2	0	71	66	30,11	50		WSW	1	Fair.

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for July, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
	°	H.	M.	°	°	Inches.	°		Points.	Str.	
July 1	58	7	0	60	64	30,08	57		SE	2	Fair.
	75	2	0	75	67	30,06	55		SE	2	Fair.
2	58	7	0	61	64	29,82	57		W	2	Cloudy.
	75	2	0	75	66	29,81	50		WSW	2	Cloudy.
3	58	7	0	62	65	29,66	61		SSE	2	Cloudy.
	67	2	0	67	65	29,47	63		SE	1	Rain.
4	55	7	0	60	63	29,38	61		W	2	Cloudy.
	65	2	0	63	64	29,38	62		W	2	Cloudy.
5	54	7	0	59	64	29,75	64		WSW	1	Cloudy.
	70	2	0	70	64	29,78	57		SW	1	Cloudy.
6	60	7	0	63	64	29,89	62		W	1	Cloudy.
	73	2	0	73	65	29,94	51		W	1	Cloudy.
7	56	7	0	64	62	30,01	57		SW	1	Fair.
	76	2	0	76	65	29,99	52		SW	1	Fair.
8	59	7	0	66	66	29,78	60		S	2	Cloudy.
	71	2	0	71	66	29,76	65		S	1	Cloudy.
9	55	7	0	61	64	29,93	61		S	2	Cloudy.
	72	2	0	72	66	29,89	52		S	2	Cloudy.
10	60	7	0	64	64	29,74	64		S	2	Cloudy.
	73	2	0	70	65	29,74	57		S	2	Fair.
11	61	7	0	64	65	29,62	64		S	2	Cloudy.
	78	2	0	77	68	29,53	54		ESE	2	Cloudy.
12	61	7	0	63	66	29,53	63		SW	2	Fair.
	73	2	0	72	66	29,58	56		SW	1	Cloudy.
13	60	7	0	65	65	29,59	60		WSW	2	Cloudy.
	71	2	0	69	66	29,57	58		WSW	2	Fair.
14	57	7	0	62	65	29,69	60		W	2	Fair.
	71	2	0	61	66	29,76	63		W	2	Rain. [Thunder and lightning.
15	52	7	0	59	64	29,92	62		NW	1	Cloudy.
	71	2	0	68	65	29,96	54		W	1	Fine.
16	51	7	0	58	64	30,08	63		W	1	Cloudy.
	70	2	0	65	65	30,03	54		W	1	Cloudy.

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for July, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.		Winds.		Weather.
		H.	M.	°	°	Inches.			Points.	Str.	
	°						°				
July 17	53	7	0	57	63	29,80	63		N	1	Cloudy.
	68	2	0	68	63	29,77	54		W	1	Cloudy.
18	54	7	0	57	62	29,55	66		E	2	Cloudy.
	67	2	0	65	63	29,66	54		N	2	Cloudy.
19	56	7	0	58	63	29,83	62		NNW	1	Cloudy.
	69	2	0	68	64	29,83	52		W	1	Cloudy.
20	53	7	0	57	63	29.78	62		NNW	1	Fair.
	68	2	0	66	64	29,78	54		N	1	Fair.
21	54	7	0	57	63	29.92	58		NW	1	Fair.
	62	2	0	62	64	30,00	55		NW	1	Fair.
22	52	7	0	56	62	30,09	57		NNW	1	Fair.
	66	2	0	65	63	30,11	52		WNW	1	Cloudy
23	52	7	0	56	62	30,16	61		SSW	1	Fair.
	73	2	0	71	64	30,16	50		W	1	Fair.
24	55	7	0	59	63	30,14	63		WSW	1	Fair.
	73	2	0	73	65	30,07	52		S	1	Fine.
25	54	7	0	60	63	29,64	59		S	1	Fair.
	76	2	0	72	67	29.78	55		SE	2	Cloudy.
26	61	7	0	65	65	29,78	62		SSW	1	Fair.
	72	2	0	72	65	29.78	58		SW	1	Rain.
27	55	7	0	60	64	29,[illegible]2	64		S	2	Cloudy.
	67	2	0	67	64	29,3[illegible]	64		SSE	2	Rain.
28	54	7	0	68	63	29,55	65		WSW	1	Fair.
	70	2	0	62	63	29,01	62		W	1	Cloudy.
29	53	7	0	60	63	29,90	64		W	1	Fair.
	70	2	0	70	64	29,94	56		W	1	Cloudy.
30	58	7	0	61	63	29.71	64		WSW	2	Fair.
	69	2	0	63	64	29,68	58		W	2	Cloudy.
31	54	7	0	60	62	29,81	61		W	2	Fair.
	71	2	0	65	64	29,80	60		SW	2	Cloudy.

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for August, 1810.

1810	Six's Therm least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.		Winds.		Weather.
	°	H.	M.	°	°	Inches.	°		Points	Str.	
Aug. 1	52	7	0	60	60	29 80	60		W	2	Fair.
	68	2	0	64	62	29 73	58		NW	2	Cloudy.
2	53	7	0	57	60	29 94	66		W	2	Fair.
	71	2	0	71	64	29,94	56		W	1	Cloudy.
3	60	7	0	63	62	29 84	67		NW	2	Cloudy.
	71	2	0	71	65	29,79	57		WNW	2	Cloudy.
4	57	7	0	61	62	29 58	67		ESE	2	Cloudy.
	68	2	0	67	63	29,53	58		S	2	Cloudy.
5	55	7	0	60	63	29,72	64		W	2	Fair.
	70	2	0	65	64	29 65	63		SSW	2	Rain. [Thunder.
6	52	7	0	57	62	29 73	64		W	2	Cloudy.
	71	2	0	70	65	29 76	54		WSW	2	Cloudy.
7	52	7	0	60	62	29.65	70		S	2	Rain.
	70	2	0	70	64	29 61	59		WSW	2	Cloudy.
8	60	7	0	62	64	29 54	67		W	1	Rain.
	70	2	0	70	65	29,51	61		W	1	Cloudy.
9	55	7	0	57	63	29 98	64		WNW	2	Cloudy.
	67	2	0	65	63	30,02	54		WNW	2	Cloudy.
10	59	7	0	62	62	29,77	72		W	2	Cloudy.
	71	2	0	71	65	29,77	51		W	2	Fair.
11	55	7	0	58	59	29,59	63		W	2	Fair.
	70	2	0	62	62	29.57	59		WSW	2	Cloudy.
12	59	7	0	63	63	29,52	64		W	2	Cloudy.
	70	2	0	70	65	29,56	54		W	2	Fair.
13	59	7	0	64	63	29,47	65		NW	2	Cloudy.
	68	2	0	67	64	29,69	52		WNW	2	Cloudy.
14	57	7	0	62	63	29,80	64		SW	2	Cloudy.
	67	2	0	64	63	29,78	62		SW	2	Cloudy.
15	58	7	0	61	63	29,05	60		W	1	Cloudy.
	69	2	0	68	63	29.49	54		W	2	Cloudy.
16	53	7	0	53	62	29,48	70		W	2	Rain.
	56	2	0	56	62	29.63	66		N	2	Cloudy.

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for August, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
		H.	M.	°	°	Inches.			Points.	Str.	
Aug. 17	50°	7	0	52	60	29,84	64°		W	1	Cloudy.
	63	2	0	62	62	29,94	74		NW	1	Fair.
18	48	7	0	51	60	30,18	65		W	1	Cloudy.
	69	2	0	67	62	30,20	54		W	1	Fine.
19	54	7	0	58	62	30,17	67		W	1	Cloudy.
	67	2	0	67	62	30,17	67		W	1	Cloudy.
20	51	7	0	52	61	30,28	66		W	1	Fair.
	71	2	0	69	63	30,27	56		W	1	Fine.
21	53	7	0	55	62	30,22	68		W	1	Fair.
	70	2	0	70	65	30,20	56		W	1	Fine.
22	53	7	0	54	63	30,14	68		W	1	Fair.
	74	2	0	74	66	30,05	57		SSW	1	Fine.
23	57	7	0	60	63	29,99	67		N	1	Hazy.
	78	2	0	76	69	29,99	56		S	2	Fine.
24	59	7	0	61	64	30,03	66		ESE	1	Hazy.
	79	2	0	78	67	30,12	55		SE	1	Fine.
25	60	7	0	63	66	29,99	65		ENE	1	Hazy.
	80	2	0	80	71	29,97	56		WSW	2	Fine.
26	61	7	0	63	65	29,95	67		W	1	Hazy.
	73	2	0	73	67	29,95	58		W	2	Cloudy.
27	57	7	0	61	64	30,07	63		N	1	Cloudy.
	74	2	0	73	67	30,07	59		W	2	Cloudy.
28	50	7	0	59	66	30,14	63		W	1	Hazy.
	72	2	0	71	67	30,16	53		N	1	Fine.
29	56	7	0	58	66	30,13	65		SSE	1	Fair.
	72	2	0	70	67	30,10	55		WSW	1	Fair.
30	61	7	0	62	66	30,01	66		W	1	Hazy.
	77	2	0	77	69	29,97	55		WSW	1	Cloudy.
31	61	7	0	62	67	29,88	66		E	1	Cloudy [Th. and L. last night.
	80	2	0	80	70	29,88	57		SSE	1	Fine.

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for September, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
		H.	M.	°	°	Inches.			Points.	Str.	
Sep. 1	° 66	7	0	66	69	29,86	° 67		E	1	Fair.
	81	2	0	80	75	29,86	57		SE	1	Fine.
2	67	7	0	68	70	29,91	66		W	1	Fair.
	83	2	0	83	76	29,89	53		E	2	Fine.
3	65	7	0	66	70	29,83	65		W	2	Cloudy.
	75	2	0	72	71	29,82	55		W	2	Cloudy.
4	57	7	0	59	68	29,66	69		W	1	Cloudy.
	65	2	0	62	63	29,74	58		WNW	2	Cloudy.
5	51	7	0	53	66	30,00	65		W	1	Hazy.
	66	2	0	66	68	30,03	54		W	1	Fine.
6	56	7	0	58	66	29,98	66		W	1	Fair.
	69	2	0	66	65	30,03	58		NW	1	Cloudy.
7	49	7	0	52	64	30,33	63		N	1	Fair.
	64	2	0	64	66	30,34	55		E	1	Fine.
8	49	7	0	52	64	30,20	63		N	1	Fair.
	67	2	0	67	66	30,13	55		E	1	Fine.
9	50	7	0	52	61	30,08	65		SE	1	Fair.
	70	2	0	69	65	30,03	57		SW	1	Fine.
10	53	7	0	55	62	29,94	67		W	1	Cloudy.
	70	2	0	70	65	29,92	60		W	1	Cloudy.
11	54	7	0	54	63	29,82	59		WSW	1	Cloudy.
	62	2	0	62	63	29,79	60		S	1	Rain.
12	50	7	0	52	60	29,64	79		NE	1	Rain.
	59	2	0	59	61	29,77	62		SE	1	Cloudy.
13	48	7	0	51	60	30,07	65		NW	1	Cloudy.
	64	2	0	63	62	30,07	52		W	1	Cloudy.
14	57	7	0	59	61	30,12	73		W	1	Cloudy.
	70	2	0	70	64	30,17	57		NE	1	Fair.
15	54	7	0	54	61	30,36	62		ENE	2	Fair.
	62	2	0	62	61	30,36	53		E	1	Fair.
16	53	7	0	55	60	30,27	62		NE	1	Fair.
	65	2	0	64	61	30,23	56		E	1	Cloudy.

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for September, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
		H.	M.	°	°	Inches.			Points.	Str.	
Sep. 17	61°	7	0	60	61	30,09	71°		NNE	1	Cloudy.
		2	0	69	62	30,07	63		ESE	1	Cloudy.
18		7	0	57	60	30 02	70		NE	2	Fair.
		2	0	67	62	30,02	65		ENE	1	Cloudy.
19		7	0	59	62	30 08	72		N	1	Cloudy.
		2	0	66	63	30,09	63		N	1	Fair.
20		7	0	56	61	30,09	71		E	1	Foggy.
	62	2	0	62	62	30,08	71		E	1	Cloudy.
21	57	7	0	59	62	30,07	72		N	1	Cloudy.
	69	2	0	68	63	30.05	61		E	1	Fair.
22	57	7	0	57	62	30,00	73		SE	1	Foggy.
	70	2	0	70	64	29,92	63		E	1	Cloudy.
23	53	7	0	55	62	29.89	65		S	2	Cloudy.
	61	2	0	61	62	29,92	58		SE	1	Cloudy.
24	54	7	0	55	61	30,05	71		N	1	Cloudy.
	67	2	0	66	62	30,07	55		NNE	1	Cloudy.
25	58	7	0	58	62	30,13	70		N	2	Cloudy.
	71	2	0	71	64	30,11	56		E	1	Fine.
26	55	7	0	57	62	30,08	72		NNE	1	Hazy.
	69	2	0	68	64	30,06	51		E	2	Fine.
27	53	7	0	55	63	29.98	71		N	1	Hazy.
	68	2	0	68	65	29.92	54		E	1	Fine.
28	54	7	0	56	63	29,89	72		N	1	Hazy.
	71	2	0	71	66	29,93	59		NNE	1	Fine.
29	50	7	0	53	62	30,05	68		W	1	Foggy.
	67	2	0	67	65	30,06	61		S	1	Fine.
30	58	7	0	61	63	29.97	71		E	1	Fair.
	70	2	0	70	66	29,98	63		SE	2	Fine.

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for October, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
		H.	M.	°	°	Inches.			Points.	Str.	
Oct. 1	° 60	7	0	61	61	30,13	° 69		E	1	Cloudy.
	68	2	0	68	64	30,16	62		E	1	Fine.
2	51	7	0	56	63	30,24	69		NE	2	Fair.
	66	2	0	65	65	30,25	61		ESE	2	Fine.
3	49	7	0	53	62	30,24	68		NE	1	Fine.
	65	2	0	66	64	30,24	55		E	2	Fine.
4	50	7	0	52	62	30,25	71		NE	1	Fine.
	67	2	0	67	64	30,25	60		ESE	2	Fine.
5	50	7	0	51	62	30,16	70		NE	1	Fine.
	65	2	0	65	64	30,07	57		SE	2	Fine.
6	50	7	0	54	61	29,93	72		SW	1	Foggy.
	60	2	0	60	63	29,93	68		SW	1	Fine.
7	48	7	0	48	60	29,96	71		WSW	1	Foggy.
	66	2	0	66	62	29,95	68		W	1	Fine.
8	51	7	0	54	59	29,96	71		W	1	Foggy.
	66	2	0	71	61	29,96	60		ESE	2	Fine.
9	53	7	0	54	61	29,92	74		NE	1	Rain.
	63	2	0	64	62	29,89	62		E	2	Fine.
10	55	7	0	56	60	29,83	66		N	1	Cloudy.
	61	2	0	58	60	29,84	63		ESE	1	Cloudy.
11	52	7	0	53	60	29,92	66		NNE	1	Cloudy.
	60	2	0	62	61	29,92	58		SSE	1	Fine.
12	47	7	0	49	58	29,85	62		NE	1	Fair.
	57	2	0	66	60	29,85	56		NE	1	Fine.
13	41	7	0	45	57	30,00	61		NNE	2	Fair.
	56	2	0	63	59	30,05	56		NE	2	Fine.
14	41	7	0	46	56	30,23	63		N	1	Cloudy.
	59	2	0	57	57	30,23	56		ESE	2	Fair.
15	45	7	0	47	55	30,19	61		E	2	Fair.
	56	2	0	59	57	30,10	55		E	1	Fine.
16	44	7	0	45	55	29,89	62		E	1	Fair.
	55	2	0	54	56	29,83	62		SE	1	Cloudy.

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for October, 1810.

1810	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.		Winds. Points.	Winds. Str	Weather.
Oct. 17	° 54	7	0	57	57	29.65	° 78		NE	1	Fair.
	64	2	0	61	58	29,57	65		SE	1	Cloudy.
18	55	7	0	54	57	29,35	73		W	1	Cloudy.
	64	2	0	63	59	29.46	60		WSW	1	Fair.
19	48	7	0	49	57	29,80	65		N	1	Rain.
	60	2	0	58	58	29,79	62		SW	1	Cloudy.
20	58	7	0	58	58	29,72	66		SW	1	Cloudy.
	65	2	0	60	61	29,66	64		SW	1	Rain.
21	57	7	0	54	56	29,59	70		WSW	1	Rain.
	64	2	0	64	59	29,56	71		SW	2	Rain.
22	58	7	0	58	59	29,30	66		W	2	Cloudy.
	61	2	0	60	59	29,30	56		W	1	Fair.
23	49	7	0	50	57	29,63	61		W	1	Hazy.
	57	2	0	57	61	29,62	56		W	2	Fair.
24	45	7	0	47	58	29,67	62		W	1	Hazy.
	53	2	0	52	60	29,83	59		NNW	1	Fair.
25	42	7	0	42	57	30,14	64		NW	1	Hazy.
	52	2	0	52	57	30,21	60		NNE	1	Fair.
26	40	7	0	41	56	30,35	67		N	1	Cloudy.
	52	2	0	50	57	30,38	57		NE	2	Fair.
27	45	7	0	45	55	30,23	60		NE	1	Hazy.
	49	2	0	49	57	30,14	59		NNE	1	Cloudy.
28	39	7	0	45	55	29.71	69		WSW	1	Rain.
	51	2	0	49	55	29.57	77		N	1	Rain.
29	38	7	0	40	53	29.59	67		N	1	Hazy.
	44	2	0	44	55	29,71	55		N	2	Fine.
30	34	7	0	38	52	29,79	69		N	1	Fair.
	44	2	0	44	53	29.94	62		N	1	Fair.
31	30	7	0	34	51	30,02	64		W	1	Hazy.
	45	2	0	43	51	29.99	64		WSW	1	Cloudy.

METEOROLOGICAL JOURNAL

for November, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.		Winds.		Weather.
		H.	M.	°	°	Inches.			Points.	Str.	
	°						°				
Nov. 1	43	7	0	43	51	29,74	73		W	1	Cloudy.
	51	2	0	48	53	29,67	67		NNE	1	Cloudy.
2	40	7	0	41	50	29,73	67		NNE	2	Cloudy. [Much wind last night.
	*	2	0	47	53	29,81	65		NE	2	Cloudy.
3		7	0	43	51	29,95	67		NE	2	Cloudy.
		2	0	45	54	29,97	69		E	2	Cloudy.
4		7	0	42	51	29,87	73		ENE	1	Rain.
		2	0	45	50	29,80	68		N	1	Cloudy.
5		7	0	36	48	29,67	68		NW	1	Hazy.
		2	0	40	52	29,55	66		W	1	Cloudy.
6		7	0	38	49	29,16	72		W	1	Hazy.
		2	0	42	53	29,10	69		WSW	1	Cloudy.
7		7	0	40	50	28,94	71		E	1	Rain.
		2	0	41	52	28,95	70		E	1	Cloudy.
8		7	0	41	51	29,16	73		E	1	Hazy.
		2	0	46	54	29,05	73		SSE	1	Fair.
9		7	0	38	51	29,44	73		W	1	Hazy.
		2	0	55	53	29.48	68		S	1	Cloudy.
10		7	0	43	51	28,87	77		E	2	Rain.
		2	0	46	54	28,61	79		SW	1	Cloudy.
11		7	0	45	52	29,19	73		W	1	Hazy.
		2	0	45	53	29,34	70		NW	2	Cloudy.
12		7	0	43	51	29,53	72		NE	1	Cloudy.
		2	0	45	53	29,68	65		NNE	1	Cloudy.
13		7	0	37	51	30,04	71		NNE	1	Cloudy.
		2	0	47	53	30,15	64		ENE	1	Fair.
14		7	0	38	51	30,08	63		ENE	2	Cloudy.
		2	0	40	51	29,86	63		E	2	Cloudy.
15		7	0	40	52	29,47	78		W	1	Cloudy.
		2	0	55	55	29,51	62		WSW	1	Cloudy.
16		7	0	50	55	29,24	72		WSW	2	Cloudy.
		2	0	57	58	29,27	67		SW	1	Fair.

* Six's Thermometer blown down and broken.

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for November, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.		Winds.		Weather.
		H.	M.	o	o	Inches.			Points.	Str.	
	o						o				
Nov. 17		7	0	48	56	29,29	68		W	1	Cloudy.
		2	0	56	58	29,33	65		W	1	Fair.
18		7	0	45	56	29,51	68		W	1	Fair.
		2	0	50	57	29,57	61		W	1	Fair.
19		7	0	48	55	29,54	71		S	1	Fair.
		2	0	49	56	29,59	69		WSW	2	Cloudy.
20		7	0	45	56	29,65	69		S	1	Hazy.
		2	0	46	56	29,66	69		NE	2	Cloudy.
21		7	0	50	56	29,41	79		E	1	Rain.
		2	0	55	59	29,39	77		S	2	Cloudy.
22		7	0	48	57	29,51	67		S	2	Cloudy.
		2	0	50	58	29,65	66		S	1	Rain.
23		7	0	47	57	29,82	71		SE	1	Cloudy.
		2	0	50	60	29,44	68		SE	1	Fair.
24		7	0	49	58	29,74	73		ESE	1	Cloudy.
		2	0	50	59	29,67	73		ESE	2	Cloudy.
25		7	0	45	58	29,58	73		SSE	1	Fair.
		2	0	47	58	29,55	72		SSE	1	Rain.
26		7	0	44	55	29,34	73		ESE	2	Cloudy.
		2	0	44	58	29,26	74		E	1	Rain.
27		7	0	43	55	29,19	75		ESE	1	Foggy.
		2	0	46	59	29,05	73		E	1	Cloudy.
28		7	0	43	55	29,02	74		W	1	Cloudy.
		2	0	47	59	28,99	67		SW	1	Fair.
29		7	0	40	55	28,96	71		W	1	Cloudy.
		2	0	42	55	28,96	75		SSW	2	Cloudy.
30		7	0	37	54	29,13	72		W	1	Cloudy.
		2	0	42	53	29,10	74		ESE	2	Rain.

METEOROLOGICAL JOURNAL

for December, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
		H.	M.	°	°	Inches.			Points.	Str.	
	°						°				
Dec. 1		8	0	37	53	29.50	71		W	1	Foggy.
		2	0	42	56	29.58	63		SW	1	Fair.
2		8	0	35	51	29.95	67		N	1	Fair.
		2	0	40	53	29,93	68		N	1	Fine.
3		8	0	33	49	29.94	70		SE	1	Fair.
		2	0	40	50	29.88	72		E	1	Rain.
4		8	0	40	49	29.91	78		SW	1	Foggy.
		2	0	47	51	29,93	76		WSW	1	Fair.
5		8	0	45	50	29,92	78		W	1	Fair.
		2	0	49	54	29,92	74		W	1	Cloudy.
6		8	0	47	53	29,59	63		SSW	2	Rain.
		2	0	49	54	29,56	75		S	1	Cloudy.
7		8	0	45	53	29,48	73		W	1	Cloudy.
		2	0	47	54	29,31	68		W	1	Cloudy.
8		8	0	39	53	29,47	70		W	1	Cloudy.
		2	0	40	54	29,60	71		WSW	1	Rain.
9		8	0	33	50	29,83	68		W	1	Cloudy.
		2	0	37	52	29,86	66		W	1	Cloudy.
10		8	0	37	49	29,61	71		E	1	Rain.
		2	0	47	50	29,20	75		SW	2	Rain.
11		8	0	36	48	29,50	74		NNW	1	Cloudy.
		2	0	39	51	29,73	66		N	2	Fair.
12		8	0	35	48	29,75	70		E	1	Cloudy.
		2	0	49	51	29,29	76		SW	1	Rain.
13		8	0	45	48	29,74	74		W	1	Cloudy.
		2	0	47	54	29,94	67		S	1	Cloudy.
14		8	0	49	53	29,64	75		W	1	Cloudy.
		2	0	47	54	29,03	60		W	1	Cloudy.
15		8	0	42	52	29,83	69		W	1	Cloudy.
		2	0	46	53	29,89	67		WNW	1	Fair.
16		8	0	41	51	30,18	59		N	1	Cloudy.
		2	0	41	51	30,27	70		W	1	Foggy.

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for December, 1810.

1810	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.		Winds.		Weather.
		H.	M.	°	°	Inches.			Points.	Str.	
	°						°				
Dec. 17		8	0	45	52	30,14	73		W	1	Fogg.
		2	0	46	52	29,68	69		W	1	Cloudy.
18		8	0	47	52	29,63	77		S	1	Rain.
		2	0	47	52	29,58	70		WSW	1	Rain.
19		8	0	42	50	29,46	68		W	1	Cloudy.
		2	0	42	52	29,40	68		W	1	Rain.
20		8	0	36	49	29,35	69		W	1	Fair.
		2	0	46	53	29,46	70		W	1	Fair.
21		8	0	42	51	29,32	69		W	1	Rain.
		2	0	41	51	29,28	65		W	1	Fair.
22		8	0	41	50	29,62	65		W	1	Rain.
		2	0	46	52	29,66	70		SW	1	Rain.
23		8	0	47	52	29,38	69		W	2	Rain.
		2	0	48	52	29,43	66		W	1	Fair.
24		8	0	43	50	29,26	73		N	1	Rain.
		2	0	43	50	29,46	71		W	1	Fair.
25		8	0	47	50	28,98	72		SW	2	Rain.*
		4	0	45	50	29,03	66		W	1	Cloudy.
26		8	0	43	48	29,51	66		W	1	Fair.
		4	0	47	52	29,77	62		W	1	Cloudy.
27		8	0	47	51	29,39	72		SW	2	Rain.
		4	0	45	52	29,58	61		W	1	Fair.
28		8	0	39	50	30,04	62		NNW	1	Cloudy.
		4	0	39	51	30,16	61		NNW	1	Fair.
29		8	0	33	48	30,35	61		NNW	1	Fair.
		4	0	37	50	30,60	63		NW	1	Fair.
30		8	0	32	47	30,50	64		N	1	Fair.
		4	0	37	47	30,45	67		N	1	Fair.
31		8	0	31	45	30,49	65		NE	1	Fair.
		4	0	32	48	30,44	64		N	1	Fair.

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* A violent gale of wind in the night.

1810.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	51	14	36,3	51	18	36,6	57	42	49,2	30,44	29,73	30,12	75	60	70,7	
February	54	18	40,6	54	19	40,8	56	43	50,8	30,44	29,01	29,86	78	57	69,1	
March	59	28	44,1	58	30	44,6	61	50	54,9	30,05	28,84	29,69	76	49	64,4	
April	69	31	48,7	69	35	49,9	62	50	55,1	30,21	29,29	29,80	76	46	62,2	
May	67	34	52,3	67	40	53,7	61	53	57,5	30,38	29,35	29,87	75	48	58,1	
June	78	45	61,5	78	52	62,0	66	59	62,0	30,38	29,68	30,07	64	48	56,7	
July	78	51	63,1	77	56	64,7	68	62	64,2	30,16	29,31	29,79	66	50	58,8	
August	80	50	63,3	80	51	64,3	71	59	63,9	30,28	29,45	29,73	72	51	61,3	
September				72	51	62,1	76	60	63,9	30,36	29,64	30,02	79	51	63,2	
October				71	34	54,1	65	51	56,9	30.38	29,30	29,91	78	55	63,9	
November				57	36	45,1	60	48	52,6	30,15	28,61	29,45	79	63	70,2	
December				49	31	40,5	56	45	51,0	30,60	28,98	29.72	78	61	69,1	
Whole year						51,5			56,8			29,84			64,0	

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXI.

PART II.

LONDON,

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MDCCCXI.

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XI. *On the Causes which influence the Direction of the Growth of Roots. By* T. A. Knight, *Esq. F. R. S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read March 7, 1811.

I HAVE shewn, in a former communication, the effects of centrifugal force upon germinating seeds; from which I have inferred that the radicles are made to descend towards the earth, and the germs, or elongated plumules, to take the opposite direction, by the influence of gravitation; and I believe the facts I have stated to be sufficient to support the inferences I have drawn.* But the fibrous roots of plants, being much less succulent, though not uninfluenced in the directions they take by gravitation, are, to a great extent, obedient to other laws, and are generally found to extend themselves most rapidly, and to the greatest length, in whatever direction the soil is most favourable: whence many naturalists have been disposed to believe that these are guided by some degrees of feeling and perception, analogous to those of animal life.

I shall proceed to state some of the facts upon which this hypothesis has been founded, and others which have occurred in the course of my own experience, and which are favourable to it; after which I shall endeavour to trace the effects observed to the operation of different causes.

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* Phil. Trans. 1806, 1st Part, page 5.

When a tree, which requires much moisture, has sprung up, or been planted, in a dry soil, in the vicinity of water, it has been observed, that much the largest portion of its roots has been directed towards the water; and that when a tree of a different species, and which requires a dry soil, has been placed in a similar situation, it has appeared, in the direction given to its roots, to have avoided the water and moist soil.

A tree growing upon a wall, at some distance from the ground, and consequently ill supplied with food and water, has also been observed to adapt its habits to its situation, and to make very singular and well directed efforts to reach the soil beneath, by means of its roots.* During the period in which it is making such efforts, little addition is made to its branches, and almost the whole powers of the plant appear to be directed to the growth of one or more of its principal roots. To these much is in consequence annually added, and they proceed perpendicularly towards the earth, unless made to deviate by some opposing body: and as soon as the roots have attached themselves to the soil, the branches grow with vigour and rapidity, and the plant assumes the ordinary habits of its species.

DU HAMEL caused two trenches to be made so as to intersect each other at right angles, and a tree to be planted at the point of intersection; and taking up this tree some years afterwards, he found that the roots had almost wholly confined themselves to the trenches, in which the soil of the former surface must have been buried.

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* SMITH's Introduction to Botany.

A trench which was twenty feet long, six wide, and about two deep, was prepared in my garden, in the bottom of which trench was placed a layer, about six inches deep, of very rich mould, incorporated with much fresh vegetable matter. This was covered, eighteen inches deep, with light and poor loam, and upon the bed thus formed, seeds of the common carrot (*Daucus carota*) and parsnip (*Pastinaca sativa*) were sowed. The plants grew feebly till near the end of the summer, when they assumed a very luxuriant growth, grew rapidly till late in the autumn, and till their leaves were injured by frost. The roots were then examined, and were found of an extraordinary length, and in form almost perfectly cylindrical, having scarcely emitted any lateral fibrous roots into the poor soil, whilst the rich mould beneath was filled with them.

In another experiment of the same season, the preceding process was reversed, the rich soil being placed upon the surface, and the poor beneath. The plants here grew very luxuriantly, and acquired a considerable size early in the summer; and when the roots were taken up in the autumn, they were found to have assumed very different forms. The greater part had divided into two or more unequal ramifications, very near the surface of the ground, and those which were not thus divided tapered rapidly to a point at the surface of the poor soil, into which few of their fibrous roots had entered.

In other experiments seeds of almost all the common esculent plants of a garden were so placed that the young plants had an opportunity of selecting either rich, or poor soil;

which was disposed, in almost every possible way, within their reach; and I always found abundant fibrous roots in the rich soil, and comparatively few in the poor.

The following experiment afforded the most remarkable result, and one the least favourable to the hypothesis which I have advanced in a former Paper,* and to the conclusion which I shall now endeavour to support; and therefore I think it necessary to describe it very minutely. Some seeds of the common bean (*Vicia faba*), the plant with which many former experiments were made, were placed upon the surface of the mould in garden pots, in rows which were about four inches distant from each other. A grate, formed of slender bars of wood, was then adapted to the surface of each pot, so as to prevent both the mould and the seeds falling out, in whatever position the pots might be placed; and the bars were so disposed, as not at all to interfere with the radicles of the seeds, when protruding. The pots were then directly inverted; and the seeds were consequently placed beneath the mould; but each seed was so far depressed into the mould, as to be about half covered: by which means each radicle, when first emitted, was in contact with the mould above, and the air below. Water was then introduced through the bottom of the inverted pot, in sufficient quantity to keep the mould moderately moist; and, the pots being suspended from the roof of a forcing house, the seeds soon vegetated.

In former experiments,† wherever the seeds were placed to vegetate at rest, the radicles descended perpendicularly downwards, in whatever direction they were first protruded; but under the preceding circumstances they extended horizontally along the surface of the mould, and in contact with it; and in a few days emitted many fibrous roots upwards into it: just as they would have done, if guided by the instinctive faculties and passions of animal life; and as I concluded before I made the experiment that they would do, under the guidance of much more simple laws, whose mode of operating I shall endeavour to explain.

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* Phil. Trans. 1806, page 1.

† Ibid.

Whatever be the machinery by which the sap of trees is raised to the extremities of their branches, it is obvious that this machinery is first put into action by the stems and branches, and not by the roots: for the graft or bud, whenever it has become fully united to the stock, wholly regulates the season and temperature, in which the sap is to be put in motion, in perfect independence of the habits of the stock; whether those be late or early. If all the branches of a tree, exclusive of one, be much shaded by contiguous trees,* or other objects, the branch which is exposed to the light attracts to itself a large portion of the ascending sap, which it employs in the formation of leaves and vigorous annual shoots, whilst the shaded branches become languid and unhealthy. The motion of the ascending current of sap appears therefore to be regulated by the ability to employ it in the trunk and branches of the tree; and this current passes up through the alburnum, from which substance the buds and leaves spring. But the sap which gives existence to, and feeds the root, descends through the bark:† and if the operation of light give ability to the exposed branch to attract and employ the ascending or alburnous current of sap, it appears not improbable that the operation of proper food and moisture in the soil, upon the bark of the root, may give ability to that organ to attract and employ the descending, or cortical current of sap; and if this be the case, an easy explanation of all the preceding phenomena immediately presents itself.

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* Phil. Trans. 1805 and 1809, p. 8.

† Phil. Trans. 1809, 1st Part, p. 1.

A tree growing upon a wall, and unconnected with the earth, will almost of necessity grow slowly, and as it must be scantily supplied with moisture during the summer, it will rarely produce any other leaves than those which the buds contained, which were formed in the preceding year. Some of the roots of a tree, thus circumstanced, will be less well supplied with moisture than others, and these will be first affected by drought: their points will in consequence become rigid and inexpansible, and they will thence generally cease to elongate at an early period of the summer. The descending current of sap will be then employed in promoting the growth and elongation of those roots only, which are more favourably situated, and those, comparatively with other parts of the tree, will grow rapidly. Gravitation will direct these roots perpendicularly downwards, and the tree will appear to have adopted the wisest and best plan of connecting itself with the ground: and it will really have employed the readiest means of doing so, as effectively as it could have done, if it had possessed all the feelings and instinctive passions and powers of animal life. The subsequent vigorous growth of such a tree is the natural consequence of an improved and more extensive pasture.

When the seeds of the carrot and parsnip, in the experiments I have stated, were placed in a poor superficial soil, but which permitted the roots of the plants to pass readily through it, these were conducted downwards by gravitation; whilst the plants grew feebly, because they received but little nutriment. The roots were in a situation analogous to that of the stems of trees in a crowded forest; and when the leading fibres of the roots came into contact with the rich mould, they acquired a situation correspondent to that of the leading branches of such trees, which are alone exposed to the light. The form of the roots of the plants was consequently long, slender, and cylindrical, like the stems of such trees. The roots of the one required the actual contact of proper soil and nutriment; and the branches of the other required the actual contact of light, to promote their growth.

When, on the contrary, the seeds of the preceding species of plants were placed in a rich superficial soil, their situation was analogous to that of a tree fully exposed, on every side, to the light, whose branches would be extended, in every direction, immediately above the surface of the ground: and as the fibrous roots of the plants came into contact with the subsoil, which was not well calculated to promote their growth, their situation became analogous to that of shaded branches; and they consequently ceased to extend downwards. The fibrous roots of a tree, under similar circumstances, would have extended along the lower surface of the favourable soil; but after these roots had much increased in bulk, they would be found partly compressed into the subsoil, however poor and unfavourable, provided it contained no ingredients actually noxious. In obedience to similar laws, the roots of an aquatic tree will not extend freely in dry soil, nor those of a tree which requires but little moisture in a wet soil; and on this account the roots of the one will appear to have sought, and those of the other to have avoided, the contiguous water;

though both, in the first period of their growth, pointed their roots alike in every direction.

When the seeds of the bean, in the experiment I have described, were placed to vegetate beneath the mould of an inverted pot, a sufficient quantity of moisture was afforded by the mould to occasion the protrusion of the radicles: but as soon as the under points of these had penetrated through the seed-coats, their surfaces were necessarily exposed to dry air, and were consequently rendered rigid and inexpansible; whilst their upper surfaces, being in contact with the moist mould, remained soft and expansible. If both the upper and lower surfaces of the radicles, at their points, had been equally well supplied with moisture, gravitation would have attracted the sap to the lower sides, where new matter would have been added; and the radicles would have extended perpendicularly downwards, as in former experiments: but the influence of gravitation was, to a great extent, counteracted by the effects of drought upon the lower sides of the radicles, nearly as it was counteracted by centrifugal force, when made to act horizontally.*

As soon as the radicles had acquired sufficient age and maturity, efforts were made by them to emit fibrous roots; when want of proper moisture on the lower sides prevented their being protruded, in any other direction, except upwards. In that direction therefore they were alone emitted, (as I was confident that they would before I began the experiment) and having found proper food and moisture in the pots, they extended themselves upwards through more than half the mould, which these contained.

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* Phil. Trans. 1806, p. 6.

This experiment was repeated, and water was so constantly and abundantly given, that every part of the radicles was kept equally wet; and they then became perfectly obedient to gravitation, without being at all influenced by the mould above them.

In other experiments pieces of alum and of the sulphates of iron and copper were placed at small distances perpendicularly beneath the radicles of germinating seeds, of different species, to afford an opportunity of observing whether any efforts would be made by them to avoid poisons; but they did not appear to be at all influenced, except by actual contact of the injurious substances. The growth of their fibrous lateral roots was, however, obviously accelerated, when their points approached any considerable quantity of decomposing vegetable or animal matter: and when the growth of the roots was retarded by want of moisture, the contiguity of water, in the adjoining mould, though not apparently in actual contact with them, operated beneficially: but I had reason to suspect that the growth of roots was, under these circumstances, promoted by actual contact with the detached and fugitive particles of the decomposing body, and of the evaporating water.

The growth and forms assumed by the roots of trees, of every species, are to a great extent, dependent upon the quantity of motion, which their stems and branches receive from winds; for the effects of motion upon the growth of the root, and of the trunk and branches, which I have described in a former memoir, are perfectly similar.* Whatever part of a root is moved and bent by winds, or other causes, an increased deposition of alburnous matter upon that part soon takes place, and consequently the roots which immediately adjoin the trunk of an insulated tree, in an exposed situation, become strong and rigid; whilst they diminish rapidly in bulk, as they recede from the trunk, and descend into the ground. By this sudden diminution of the bulk of the roots, the passage of the descending sap, through their bark, is obstructed; and it in consequence generates, and passes into many lateral roots; and these, if the tree be still much agitated by winds, assume a similar form, and consequently divide into many others. A kind of net-work composed of thick and strong roots is thus formed, and the tree is secured from the dangers to which its situation would otherwise expose it.

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* Phil. Trans. 1803, p. 7.

In a sheltered valley, on the contrary, where a tree is surrounded and protected by others, and is rarely agitated by winds, the roots grow long and slender, like the stem and branches, and comparatively much less of the circulating fluid is expended in the deposition of alburnum beneath the ground; and hence it not unfrequently happens, that a tree, in the most sheltered part of a valley, is uprooted; whilst the exposed and insulated tree, upon the adjoining mountain, remains uninjured by the fury of the storm.

In all the preceding arrangement, the wisdom of nature, and the admirable simplicity of the means it employs, are conspicuously displayed; but I am wholly unable to trace the existence of any thing like sensation or intellect in the plants: and I therefore venture to conclude, that their roots are influenced by the immediate operation and contact of surrounding bodies, and not by any degrees of sensation and passion analogous to those of animal life; and I reject the latter hypothesis, not only because it is founded upon assumptions, which cannot be granted, but because it is insufficient to explain the preceding phenomena, unless seedling plants be admitted to possess more extensive intellectual powers, than are given to the offspring of the most acute animal. A young wild-duck or partridge, when it first sees the insect upon which nature intends it to feed, instinctively pursues and catches it; but nature has given to the young bird an appropriate organization. The plant, on the contrary, if it could feel and perceive the objects of its wants, and will the possession of them, has still to contrive and form the organ by which these are to be approached. The writers who have contended for the existence of sensation in plants, appear to have been sensible of the preceding and other obstacles, and have all betrayed the weakness of their hypothesis, in adducing a few facts only which are favourable to it, and waving wholly the investigation of all others.

In the description of the preceding experiments, I fear that I have been tediously minute; but, as I have selected a few facts only from a great number, which I could have adduced, I was anxious to give as accurate and distinct a view of those I stated, as possible.

I am, dear Sir,
with great respect,
sincerely yours,

THO. AND. KNIGHT.

Downton, Jan. 15, 1811.

XII. *On the Solar Eclipse which is said to have been predicted by* THALES. *By* Francis Baily, *Esq. Communicated by* H. Davy, *Esq. Sec. R. S.*

Read March 14, 1811.

THERE is probably no fact in ancient history that has given rise to so many discussions, and to such a variety of opinions, as the solar eclipse, which (according to HERODOTUS) is said to have been predicted by THALES; and which, owing to a very singular coincidence, put an end to a furious war that raged between Cyaxares king of Media, and Alyattes king of Lydia.

According to the account given by that celebrated historian, " the contest had continued during five years, with alternate " advantages to each party: in the sixth, there was *a sort of* " *nocturnal combat.* For, after an equal fortune on both sides, " and whilst the two armies were engaging, *the day suddenly* " *became night.* THALES, the Milesian, had predicted this phe- " nomenon to the Ionians: and had ascertained the time of " the year in which it would happen. The Lydians and the " Medes, seeing that the night had thus taken the place of the " day, desisted from the combat; and both parties became " desirous of making peace."—ἐν τοῖσι πολλάκις μὲν οἱ Μῆδοι τοὺς Λυδοὺς ἐνίκησαν, πολλάκις δὲ οἱ Λυδοὶ τοὺς Μήδους· ἐν δὲ καὶ νυκτομαχίην τινὰ ἐποιήσαντο. διαφέρουσι δὲ σφι ἐπὶ ἴσης τὸν πόλεμον, τῷ ἕκτῳ ἔτεϊ συμβολῆς γενομένης, συνήνεικε ὥστε τῆς μάχης συνεστεώσης, τὴν ἡμέρην ἐξαπίνης νύκτα γενέσθαι. τὴν δὲ μεταλλαγὴν ταύτην τῆς ἡμέρης Θαλῆς ὁ Μιλήσιος τοισι Ἴωσι προηγόρευσε ἔσεσθαι, οὖρον προθέμενος ἐνιαυτὸν τοῦτον, ἐν ᾧ δὴ καὶ ἐγένετο ἡ μεταβολή. οἱ δὲ Λυδοὶ τε και οἱ Μῆδοι ἐϖεί τε εἶδον νύκτα ἀντὶ ἡμέρης γινομένην, τῆς μάχης τε επαύσαντο, και μᾶλλόν τι ἔσπευσαν καὶ ἀμφότεροι εἰρήνην ἑωϋτοῖσι γενέσθαι. HERODOTUS, Lib. I. §. 74.

The fact is here very clearly (and probably very justly) related: but, unfortunately, there is nothing, either in the statement itself, or in the contiguous passages of the work, that will enable us to determine, with any degree of accuracy, the exact *time* wherein this singular phenomenon took place. And this is the more to be regretted, because the dates of several other events, recorded by the same historian, might be more easily ascertained, if the era of this eclipse were correctly known; but which are now involved in much obscurity.

Deprived of all information from the body of the work itself, chronologists have called in the aid of astronomy to assist them in fixing the date of this remarkable appearance. For it must be evident, that if we could ascertain, by this mean, that in any solar eclipse, which happened about that period, the centre of the moon's shadow passed over the country bordering on the two contesting empires where the battle was probably fought (for HERODOTUS has likewise omitted to mention the *place* where the action occurred), we may reasonably and very fairly conclude, that that eclipse only was the one alluded to by the historian.—In this attempt, however, a great diversity of opinion has arisen; the origin of which it may be useful and entertaining here to trace. But, in order to render my subsequent remarks the more intelligible, I shall previously state the various dates that have been assigned to this event by the several authors above alluded to.

Pliny places this eclipse in the fourth year of the forty-eighth Olympiad; which answers to the year 585 B. C. (*Hist. Nat.* lib. 2, cap. 12). A similar opinion has been advanced, among the ancients, by Cicero (*De Divinat.* lib. 1, §. 49), and probably by Eudemus (*Clement. Alex. Strom.* lib. 1, p. 354). And, among the moderns, by Newton (*Chron. of Anc. King. amended*), Riccioli (*Chron. Refor.* Vol. I. p. 228), Desvignoles (*Chronol.* lib. 4, chap. 5, §. 7, &c.), and Brosses (*Mém. de l'Acad. des Belles Lettres*, Tome 21, Mém. p. 33.)

Scaliger, in two of his writings (*Animad. ad Euseb.* p. 89, and in Ολυμ. ἀναγραφή), has adopted the opinion of Pliny; but in another work (*De Emen. Temp. in Can. Isag.* p. 321), he fixes the date of this eclipse on the 1st of October, 583 B. C.

Calvisius, who was contemporary with Scaliger, thinks that it took place in the year 607 B. C. (*Opus Chron.*)

Petavius says that it happened July 9, 597 B. C. (*De Doct. Temp.* lib. 10, cap. 1): and he has been followed by Hardouin (*Dissert. de* lxx *hebdom. Dan.* §. 3), Marsham (*Chron. Canon.* p. 561), Bouhier (*Recher. et Diss. sur Hérodot.* p. 42), and Corsini (*Fast. Attic.* Tom. III. p. 68); together with M. Larcher, the French translator of Herodotus (Tom. I. p. 335).

Usher is of opinion that it happened on the 20th of September, 601 B. C. (*Annal. Vet. et Nov. Testam.*)

Bayer has shown, from the astronomical tables then in use, that this eclipse ought to have taken place May 18th, 603 B. C. (*Com. Acad. Scient. Imp. Petrop.* Tom. III.): and he has been supported in this opinion by the two English astronomers, COSTARD and STUKELEY (*Phil. Trans.* for 1753, pages 17 and 221).

Lastly, M. VOLNEY has attempted to show, in a recent publication (*Chronologie d'Hérodote*) that the eclipse, mentioned by the historian, could be no other than the one which happened February 3d, 626 B. C.

Thus we find a distance of no less than forty-three years between the extreme periods that have been assigned for this eclipse: an interval which, however, may be somewhat abridged; since there are other facts recorded by the same historian which enable us to reduce these limits, and yet leave the narration consistent with itself.

For, according to HERODOTUS, the two kings of Media, that immediately preceded the conquest of that country by Cyrus, were Cyaxares, who reigned forty years, and Astyages, who reigned thirty-five years: and it is admitted by all the chronologists, that Cyrus conquered Astyages in the year 560 B. C. Consequently (if the numbers given by HERODOTUS be correct) the reign of Cyaxares extended from 635 B. C. to 595 B. C. And, since the battle of the eclipse was fought in the sixth year of a war which *began after* Cyaxares had ascended the throne, it could not happen earlier than 629 B. C. nor later than 595 B. C. If therefore we can find, within this short space of thirty-four years, a solar eclipse that was central and *total* in that part of Asia bordering on the two hostile empires, where this battle was probably fought, we may justly conclude that it was the one alluded to by the historian.

I say that this eclipse must have been a *total* one, because no *annular* eclipse (and much less a *partial* one) could have produced that degree of obscurity alluded to by HERODOTUS. The celebrated MACLAURIN, in his account of the *annular* eclipse which happened at Edinburgh, February 18th, 1737, observes (*Phil. Trans.* Vol. XL, p. 177), that "during the "appearance of the annulus, the direct light of the sun was "still *very considerable*; but the places that were shaded from "his light, appeared gloomy:"—that "day-light was *not* "*greatly obscured;* appearing only so much dimmer than "usual, as that of the sun is, when seen through a gentle "mist in a fine morning in April or May." And, as a further proof of the trifling alteration this phenomenon made, he observes, that "there was little notice taken of this eclipse by the "populace in the country: and I cannot but add, that several "gentlemen of very good credit, and not in the least short-"sighted, assure me, that about the *middle* of the annular ap-"pearance they were *not able to discover the moon upon the sun*, "when they looked without a smoked glass, or something "equivalent." In another account likewise of this eclipse, in the same volume, by Sir JOHN CLERK, Bart. it is observed that "there was *no considerable darkness;* but the ground was co-"vered with a kind of dark greenish colour." And M. LE MONNIER (who came over from France on purpose to observe the *annular* eclipse of the sun, which happened July 14th, 1748) says, "that when he looked at the sun with his naked eyes, "during the middle of the eclipse, he could observe *nothing* "*upon the sun*, but saw the sun *full*, though faint in his light." (*Phil. Trans.* Vol. XLV. p. 588).

In the account also which is given, in the *Mémoires de l'Acad. Roy. des Sciences* for 1724, of the *total* eclipse which happened on the 22d of May in that year, it is stated that, at the moment when the *last* portion of the sun was covered by the moon, "la clarté a diminué *tout d'un coup;* de sorte qu'on a eû be-"soin de lumière pour compter à la pendule: on voyoit les "personnes au grand air, mais on ne distinguoit pas bien les "visages à quelques pas de loin." In another account, in the same volume, it is stated, that the darkness came on *dans un instant;* and that, after an interval of two minutes and sixteen seconds "le soleil *commença* à reparoitre *comme un éclair,* "qui dissipa *sur le champ* les tenébres dans lesquelles on étoit "plongé." M. DESVIGNOLES, likewise (in his *Chronologie de l'Histoire Sainte,* Vol. II. p. 253), gives an extract of a letter from M. ABAUZIT of Geneva, who, at the close of his remarks on the calculation of PETAVIUS respecting this very eclipse, observes "il ignoroit que *le moindre* raion, qui commence à "poindre, est assez fort pour dissiper les tenébres: *comme je* "*l'ai observé deux fois.*" All which may serve to explain the remarkable expression of HERODOTUS, who says την ἡμέρην ἐξαπίνης νυκτα γενέσθαι, "the day *suddenly* became night:" a passage which has been ignorantly censured by some of his commentators.

It appears to me, that an inattention to these singular facts has been the principal cause of the various opinions that have arisen respecting the time when this eclipse happened. For each chronologist, having a system of his own to support, has satisfied himself merely with ascertaining that a solar eclipse did take place in the year that he had assigned for it; and which eclipse he supposed might be visible in that part of the world bordering on the two hostile countries: but without taking into his account the *magnitude* of the eclipse at the place where the battle is supposed to have been fought. Now since the territories of the two belligerent powers were probably separated by the river Halys (which was the case in the subsequent reign, although we have no authentic information that it was so at the period now under consideration), and as the battle was probably fought on the confines of these two empires, I think it will be evident from the preceding extracts, that no solar eclipse could be the one mentioned by Herodotus, unless it was central and *total* in some part of Asia Minor; that is, the centre of the moon's shadow, in such total eclipse, must have passed over that part of Asia Minor where the contending armies were engaging. Consequently the fact is capable of being verified or disproved by the present state of our knowledge in astronomy.

M. Th. S. Bayer is the first who seems to have fixed the attention of the public to this point, in a paper entitled *Chronologica Scythica*, inserted in the Petersburg Memoirs for the year 1728. He consulted his friend Fred. Chris. Mayer on this subject, who has shown, from the astronomical tables *then* in use, that neither the eclipse mentioned by Pliny, Scaliger, Calvisius, Petavius, or Usher, could possibly be the eclipse alluded to by Herodotus. For, the first two (he says) happened between the hours of sun-set and sun-rise in Asia Minor. In the third, the centre of the moon's shadow passed too near the equator, and in the last two it passed too far to the north of Asia Minor, for it to cause any remarkable obscurity there. In order, however, to set the question at rest, he calculated all the solar eclipses that could possibly be seen in Asia Minor from the year 608 B. C. to 556 B. C.; and he found that the one which took place May 18, 603 B. C. was the only one that was at all likely to be that mentioned by

HERODOTUS. According to his computation, the centre of the moon's shadow in that eclipse entered the earth's disk about N. lat. 1° 40′, and E. long. 23° from Ferrol. It proceeded then towards the mouths of the Nile; and, traversing the Mediterranean, crossed Cyprus, Cilicia, and Cappadocia, and passed over to Trebizond.

The Rev. GEORGE COSTARD, without knowing what M. MAYER had done, has drawn nearly the same conclusions; and has likewise entered into a calculation (from Dr. HALLEY's tables) of all the eclipses which have been assigned to this event by preceding authors: which may be seen, at length, in the *Philosophical Transactions* for 1753. In a subsequent paper, in the same volume, Dr. STUKELEY has given a map containing the path of the moon's shadow in this eclipse, deduced from the calculations of a Mr. WEAVER; and which correspond nearly with Mr. COSTARD's. But Mr. COSTARD has suggested an important correction in his computation, by allowing for the moon's *acceleration;* which does not appear to have been attended to either by M. MAYER or Mr. WEAVER: and which throws the route of the moon's shadow too far to the southward to pass over any part of Asia Minor. For, on this supposition (he observes) the umbra of the moon will leave Africa near Damietta; and, after traversing the south-east corner of the Mediterranean, will enter Syria between Tripoli and Tyre: and, proceeding across Mesopotamia, between Nisibin and Mosul, will enter the Caspian sea near Ardebil. Notwithstanding this circumstance, however, the date here assigned has continued to be received as the true date of the battle of the eclipse by all succeeding chronologists; although it must be evident, even from these data, that such eclipse could not be total any where near the place where the battle was probably fought.

But none of these calculations can have much weight at the present day, since they must have been formed from tables which the subsequent improvements in astronomy have shown to be exceedingly defective and incorrect. Even the *mean* motions of the sun and moon are not given with a sufficient degree of accuracy, either in the Rudolphine or Halleian tables, to enable us to determine, with any tolerable correctness, their true *mean* place of conjunction at so remote a period: neither can the lunar *equations*, there given, be safely depended upon. The *secular variations* also are wholly omitted: and these must have an important effect in all inquiries of this kind, since they increase in proportion to the period of time elapsed.

Under these circumstances, and in order to set this question at rest, as far as it can now be done by the aid of astronomical science, I have been induced to re-calculate the elements of the several eclipses, above alluded to, from the new *Tables Astronomiques*, lately published by the *Bureau des Longitudes* in France. In these tables, the mean motions of the sun and moon are given with the greatest exactness for the most distant periods: and, by the successive labours of MAYER, MASON, and BURG, the lunar equations are carried to an astonishing correctness; which, together with the secular variations deduced from the formulæ of M. LAPLACE, enable us to determine the true place of the sun and moon with considerable accuracy for many centuries prior to the Christian era. These calculations, at full length, together with a map containing the paths of the moon's shadow in the several eclipses there alluded to, are sent with this paper for the inspection of the Members of the Royal Society, should they be desirous of entering more fully into the detail. The substance of those inquiries I shall now proceed to lay before them.

The eclipse, which is supposed to have been that alluded to by PLINY, happened May 28th, 585 B. C.: and the time of the ecliptic conjunction was at $2^h\ 38'\ 22''$ in the afternoon, *mean* time at Greenwich, or $2^h\ 46'\ 24''$ *apparent* time. The elements were as follow:

True longitude of the luminaries	1^s	29°	41′	4″
Sun's declination, north		20	23	17
—— semi-diameter			15	45
Moon's semi-diameter			16	43
—— equatorial parallax			61	13
—— horary motion from the sun			35	29
—— true latitude			12	39
—— horary motion in latitude			3	30

By a projection of this eclipse, I find that the sun was centrally eclipsed on the meridian, about the middle of the Atlantic ocean, in N. lat. $33\frac{1}{4}°$ and W. long. 43°. The centre of the moon's shadow then proceeded to the parallel of N. lat. 40°, in W. long. 13°; where, turning to the southward, it crossed Spain, and traversed the course of the Mediterranean. By a trigonometrical calculation I have ascertained that the sun set centrally eclipsed on the borders of the Red Sea in N. lat. 28° 1′, and E. long. 35° 2′. So that at no time was this eclipse central in or near any part of Asia Minor. It happened likewise ten years *after* the death of Cyaxares, according to the received chronology.

With respect to the eclipse, which happened October 1st, 583 B. C. it is sufficient to observe that, as the ecliptic conjunction of the sun and moon did not take place till after four o'clock in the afternoon at Greenwich, it is evident that the sun must have set, centrally eclipsed, to the *westward* of any meridian line that can be drawn through any part of Asia Minor: and consequently the eclipse could not have been central in that peninsula.

CALVISIUS does not come much nearer the truth, in supposing that the eclipse mentioned by HERODOTUS is the one which occurred in 607 B. C. For in that which happened July 30th, the ecliptic conjunction took place at $8^h\ 26'\ 18''$ in the morning, *mean* time at Greenwich, or $8^h\ 35'\ 45''$ *apparent* time: and the elements were as follow:

True longitude of the luminaries	3^s	29°	6′	54″
Sun's declination, north		20	38	39
—— semi-diameter			15	54
Moon's semi-diameter			15	10
—— equatorial parallax			54	33
—— horary motion from the sun			27	41
—— true latitude, south			2	17
—— horary motion in latitude			2	46

By a trigonometrical calculation, I find that the sun rose centrally eclipsed off the coast of Sierra Leona in N. lat. 8° 13′ and W. long. 12° 33′. The moon's umbra then crossed the continent of Africa between the 10th and 20th degrees of north latitude: and the sun became centrally eclipsed on the meridian in Arabia Felix, in N. lat. $18\frac{1}{3}°$ and E. long. 3° 24′. It is evident, therefore, that this eclipse (independent of its being *annular*) was not central in any part of Asia Minor. The other eclipse in this year, which took place February 2d, happened when it was near midnight in Asia Minor.

The eclipse mentioned by PETAVIUS took place July 9th, 597 B. C. The ecliptic conjunction happened at $4^h\ 29'\ 25''$ in the morning, *mean* time at Greenwich, or $4^h\ 29'\ 58''$ *apparent* time: and the elements were as follow:

True longitude of the luminaries	3^s	9°	$16'$	$32''$
Sun's declination, north		23	28	18
—— semi-diameter			15	49
Moon's semi-diameter			14	50
—— equatorial parallax			54	23
—— horary motion from the sun			27	32
—— true latitude			41	59
—— horary motion in latitude			—2	44

By a trigonometrical calculation, I find that the sun rose centrally eclipsed to the inhabitants of Holland in N. lat. 51° 45′ and E. long. 5° 39′. The moon's umbra then proceeded across Denmark, Finland, and the northern provinces of Russia: and the sun became centrally eclipsed on the meridian in N. lat. 74½° and E. long. 113° 35′. This eclipse, therefore, could not possibly be the one mentioned by HERODOTUS. And yet his translator, M. LARCHER, without taking the slightest pains to verify the fact, or even to ascertain its probability, has adopted it as the most likely one, "parcequ'elle s'accorde mieux avec la chronologie que toutes les autres:" an opinion as unfounded, as the circumstance to which it relates; and an assumption which puts the visionary speculations of the antiquarian in competition with the immutable laws of nature. It is scarcely necessary to add, that this eclipse likewise was *annular.*

In the eclipse alluded to by USHER, September 20th, 601 B. C. the ecliptic conjunction took place at $7^h\ 25'\ 18''$ in the morning, *mean* time at Greenwich, or $7^h\ 31'\ 35''$ *apparent* time: and the elements were as follow:

True longitude of the luminaries	5^s	20°	$46'$	$50''$
Sun's declination, north - -		3	42	27
—— semi-diameter - -			16	8
Moon's semi-diameter - -			16	43
—— equatorial parallax -			61	14
—— horary motion from the sun			35	24
—— true latitude - -			52	1
—— horary motion in latitude			— 3	27

From a projection of this eclipse, it will be seen that the centre of the moon's shadow entered the earth's disk very near the north pole; and that the sun became centrally eclipsed on the meridian in N. lat. $73\frac{3}{4}^\circ$ and in E. long. $72^\circ\ 10'$. The umbra then passed over Siberia and the eastern parts of the Chinese empire: and consequently this eclipse was not central in any part of Asia Minor.

The eclipse first suggested by BAYER, and hitherto generally received as the true one, happened May 18th, 603 B. C. The ecliptic conjunction took place at $7^h\ 12'\ 13''$ in the morning, *mean* time at Greenwich, or $7^h\ 19'\ 36''$ *apparent* time: and the elements were as follow:

True longitude of the luminaries	1^s	19°	$15'$	$44''$
Sun's declination, north -		17	48	24
—— semi-diameter - -			15	46

Moon's semi-diameter	16′	43″
——— equatorial parallax	61	16
——— horary motion from the sun	35	32
——— true latitude	17	15
——— horary motion in latitude	3	30

By a trigonometrical calculation, I find that the sun rose centrally eclipsed in S. lat. 5° 9′ and E. long. 0° 46′. The moon's umbra then passed over the continent of Africa in a north-easterly direction; and, crossing the Red Sea, entered Arabia near Mecca, continuing its course over the provinces of Kerman and Segistan in Persia. The sun afterwards became centrally eclipsed on the meridian in N. lat. 35¼° and E. long. 68°. Consequently this eclipse could not be central in any part of Asia Minor: and yet it has generally been considered, of late years, as the only one that could be reconciled to the fact.

Lastly, I shall notice the eclipse proposed by M. VOLNEY, which happened February 3d, 626 B. C. The ecliptic conjunction took place at 4^h 19′ 27″ in the morning, *mean* time at Greenwich, or 4^h 0′ 35″ *apparent* time: and the elements were as follow:

True longitude of the luminaries	10^s	7°	47′	47″
Sun's declination, south		18	35	50
—— semi-diameter			16	7
Moon's semi-diameter			15	16
——— equatorial parallax			55	56
——— horary motion from the sun			29	13

Moon's true latitude	44	28
——— horary motion in latitude	— 2	53

By a trigonometrical calculation, I have ascertained that the sun rose centrally eclipsed to the inhabitants of Great Buccharia in N. lat. 40° 17′, and E. long. 61° 35′: and the moon's umbra then proceeded in a south-easterly direction across Thibet and China. Consequently this eclipse (which, moreover, was an *annular* one) could not possibly be central in any part of Asia lying to the west of the Caspian Sea: and M. VOLNEY ought to have taken some steps towards ascertaining this fact, before he ventured to set up his own opinion in opposition to all preceding chronologists.

I have thus shown, from the most correct evidence which the present state of astronomical science affords, that not one of the eclipses, mentioned by either of the authors above alluded to, could possibly be that which is recorded in so singular a manner by HERODOTUS. In order, however, that I might not leave the subject in the same degree of doubt in which I found it, I have taken the pains to calculate all the solar eclipses that were likely to have been visible in Asia Minor, from the year 650 B.C. to 580 B.C.: but, out of this period of seventy years, I have found only one that was central in, or *near*, any part of that peninsula.

The eclipse here alluded to, happened September 30th, 610 B.C. The ecliptic conjunction took place at $8^h\ 12'\ 51''$ in the morning, *mean* time at Greenwich, or $8^h\ 21'\ 41''$ *apparent* time: and the elements were as follow:

True longitude of the luminaries	$5^{s}\ 29^{\circ}\ 59'\ 40''$
Sun's declination, north	0 8
—— semi-diameter	16 10
Moon's semi-diameter	16 36
—— equatorial parallax	60 50
—— horary motion from the sun	34 53
—— true latitude	29 57
—— horary motion in latitude	3 26

Since the sun's declination in this eclipse was only eight seconds, it may safely be neglected in the calculation; and it may then be found very easily by plane trigonometry that the sun rose centrally eclipsed in N. lat. 47° 34′, and W. long. 11° 55′; that it was centrally eclipsed on the meridian in N. lat. 31° 6′, and E. long. 59° 33′; and set centrally eclipsed in N. lat. 11° 13′, and E. long. 122° 36′. The centre of the moon's shadow crossed the parallel of N. lat. 42° in E. long. 34° 45′; and the parallel of N. lat. 36° in E. long. 50°: and consequently passed nearly in a straight line over the north-eastern part of Asia Minor, through Armenia and Persia, where the sun became centrally eclipsed on the meridian, as above-mentioned. This eclipse, therefore, was central and total to part of Asia Minor, Armenia, and Media: and the path of the moon's umbra lay in the very track where the two hostile armies probably met. For it passed over the very mouth of the Halys, just at the point where Crœsus, the immediate successor of Alyattes, crossed that river in order to attack the Median empire.

It would appear from the order of events belonging to the reign of Cyaxares, as related by Herodotus, that the battle of the eclipse happened *prior* to the invasion of the Scythians, who kept possession of his kingdom twenty-eight years; and that, after the expulsion of those barbarians, he besieged and took the city of Nineveh, and thereby put an end to the Assyrian empire. This, however, will not accord with the date here assigned: neither indeed will it suit any of the systems above alluded to; except it be that of M. Volney, which may lay claim to some ingenuity. But his system is too much at variance with the astronomical fact to be entitled to any credit.

It has been remarked by Dr. Halley (*Phil. Trans.* Vol. XXIX. p. 245), that "though twenty-eight eclipses of the "sun happen in eighteen years, and eight pass through the "parallel of London, yet since March 20th, 1140, no *total* "eclipse has been seen in that metropolis." Indeed, so rare is this phenomenon in any particular country, that its occurrence, when well authenticated, may be considered as an era which is less liable to mistakes or confusion, than any other event recorded in history. All attempts at imposition or deceit are easily detected by our knowledge of astronomy: and the unintentional errors of the historian are soon rectified and adjusted. On this account, and as the fact of the eclipse is so confidently related by Herodotus (indeed, its singular coincidence with the battle will ever render it memorable in history), I would place the termination of the war between Alyattes and Cyaxares, in the year 610 B.C.: and, if the other events of that period, as related by the historian, cannot be reconciled to this date, I should attribute the confusion to the want of authentic documents and information at the time that the history was written.

I have before observed, that all these calculations have been made from the *Tables Astronomiques*, lately published in France: which tables have since been adapted to the meridian of Greenwich, and to astronomical time, by Mr. VINCE, and inserted by him in the third volume of his System of Astronomy.* In these tables are given the *secular variations* in the moon's mean longitude, mean anomaly, and mean distance from her node, as deduced from the formulæ of M. LAPLACE. It is with much deference that I presume to question the accuracy of the results, obtained by means of those formulæ; but, as the present subject is in a great measure connected with that inquiry, I shall briefly state my reasons for offering a doubt upon that point.

It is well known that AGATHOCLES, king of Syracuse (when besieged in that city by HAMILCAR, the Carthaginian general), undertook the bold design of invading Africa, and thereby moving the seat of war from Sicily. He accordingly embarked a numerous army, and set sail for the continent. The *day after* he left Syracuse, the fleet was terrified at an eclipse of the sun; which was so great, that, in the words of DIODORUS

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* It is to be regretted, that Mr. VINCE did not adapt his tables to the English system of *chronology* likewise. For the years before Christ, according to the *English* mode of computation, exceed by unity the corresponding years given by the *French* chronologists: since they make the year of Christ equal to 0, whereas the English reckon it as 1 B. C.—The French also assume the year 1582 as the date of the Reformation of the Calendar; whereas, in England, that event did not take place till the year 1752.

Without a proper attention to these circumstances, we may be led into an error of one whole year, in the calculation of the places of the heavenly bodies for any period prior to the Christian era; and into an error of ten or eleven days in our calculations for that space of time which is included between October 5th, 1582, and September 14th, 1752.

Siculus, lib. 20, *ὁλοσχερῶς φανῆναι νύκτα, θεωρουμένων των αστέρων πανταχȣ̃*, "it seemed *exactly* like night, the stars *every where* "appearing." This eclipse was therefore evidently *total* in the place where it was seen by the fleet of Agathocles. It happened on August 15th, 310 B. C. The ecliptic conjunction took place at 8^h 10′ 23″ in the morning, *mean* time at Greenwich, or 8^h 9′ 6″ *apparent* time: and the elements were as follow:

True longitude of the luminaries	4^s	16°	41′	32″
Sun's declination, north -		16	2	38
—— semi-diameter - -			15	57
Moon's semi-diameter - -			16	39
—— equatorial parallax -			61	0
—— horary motion from the sun			35	9
—— true latitude - -			14	42
—— horary motion in latitude -			3	28

From these elements I have found, by a trigonometrical calculation, that the sun rose centrally eclipsed to the inhabitants of the western coast of Africa, in N. lat. 24° 57′ and W. long. 14° 9′. The centre of the moon's shadow then, crossing the desert, proceeded towards the Mediterranean, near to, but rather to the southward of, Tripoli; and crossed the parallel of N. lat. 33° in E. long. 20° 44′. But in no part of its course did it advance more northerly than N. lat. 33° 55′ 36″, which I find by a trigonometrical calculation to be its maximum of latitude, and the parallel of which it reached in E. long. 35° 21′ 8″. It then turned to the south; and the sun became centrally eclipsed on the meridian in N. lat. 30¼° and E. long. 59° 45′.

Let us now compare this result, with the fact as related by DIODORUS. It is stated by this author, that AGATHOCLES was six days on his passage, from Syracuse to the coast of Africa; although he used the utmost expedition, being, in fact, closely pursued by the Carthaginian fleet. The place where he landed was called Λατομίας, the *Quarries;* whence he proceeded to the neighbouring cities of Μεγάλην πόλιν, *Megalapolis*, or the *Great City*, and Λευκὸν Τύνητα, *White Tunis*. The position of these cities is not handed down to us; all that we know is, that the latter place (which must not be confounded with the present Tunis) was two thousand stadia, or two hundred and twenty-nine English miles, distant from Carthage. AGATHOCLES, therefore, probably landed near the Syrtis Minor, or Gulph of Cabes, about three hundred miles in a direct course from Syracuse: whence we may reasonably conclude that he performed one-sixth of his passage, or about fifty miles, in the space of one day; which, I am aware, is not so much as the mean rate that has been attributed to the ships of the ancients (see HERODOTUS, lib. 4, §. 86). Syracuse lies in N. lat. 37° 3′ and E. long. 15° 14′; and, consequently, on the day after the sailing of AGATHOCLES from that port (being the day on which the eclipse took place), the fleet would be in about N. lat. 36¼°: at all events, it could not (from the direction of its course) be much farther south than this point, which is all that is required in the present instance; and a few miles, either way, not being of any material consequence. It follows therefore, that in the meridian of Syracuse, the northern part of the moon's umbra ought to extend as far north as that parallel of latitude. But, from the calculations above adduced, it will be found that the centre of the moon's shadow, on that meridian, had only reached the parallel of about N. lat. $32\frac{1}{2}°$: and as the semi-diameter of the umbra was not more than forty-seven and a quarter English statute miles, or about two-thirds of a degree, the eclipse could not *there* be total to the northward of N. lat. $33\frac{1}{4}°$. Now, since the place where AGATHOCLES landed in Africa, was probably not situated below the parallel of N. lat. 34°, it is evident that he did not, in *any part* of his course (and much less, at the *commencement* of it), come within a considerable distance of the moon's umbra.

I much doubt whether, according to our present computation, this eclipse was total even at Tripoli: and, although it was unquestionably of considerable magnitude, both there and as far north as Syracuse itself, yet (for the reasons already given in this paper) I do not think that, at any intermediate place between these two cities, it could be so great as to produce that degree of obscurity, which is recorded by DIODORUS and confirmed by JUSTIN. In order that the phenomenon should accord with the fact, as related by these historians, the centre of the moon's shadow ought to pass over, or very near to, Malta: that is, the latitude of the moon ought to be, at least, three degrees greater than our present tables make it.

Since the latitude of the moon depends on her true distance from the node, these observations (if correct) will show the necessity of some alteration in the table of the secular variation of the moon's mean distance from her node, which (agreeably to the rule given by M. LAPLACE) is deduced immediately from the secular variation of her mean longitude. These remarks, however, are thrown out merely as hints to those who are more conversant with, and better informed on, the subject: and I regret that I have not more time to pursue the inquiry farther.

Such an alteration, as is here suggested, would somewhat vary the position of the route of the moon's umbra, in all the eclipses which have been the subject of this paper; but, in none of them would it alter the conclusions which have been drawn from them, except perhaps in the one (Sept. 30th, 610 B. C.) which I have supposed to be that mentioned by HERODOTUS. In this particular case, the path of the moon's umbra might, by such a correction, be thrown so much farther north as to prevent the eclipse being total in any part of Asia Minor. But still it would remain the only one that can be at all adapted to the account given by HERODOTUS; since there is no other that could possibly be central in, or *near*, any part of Asia Minor from the year 650 B. C. to 580 B. C.: a period which far exceeds the probable limits of time wherein this singular phenomenon must have taken place, so as to be reconcileable to any received system of chronology.

F. B.

November, 1810.

XIII. *An Account of the great Derbyshire Denudation. By Mr.* J. Farey, Sen. *In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read March 21, 1811.

Sir,

I had but recently entered on the survey of Derbyshire and its environs, which under your kind patronage I was induced to commence in the autumn of 1807, and had only cursorily examined the strata, in my way from Charnwood Forest and Breedon in Leicestershire, in order to meet you at Overton Hall, before I perceived clearly, that those principles which contemplate the terrestrial strata as terminating or *ending* in one direction (simple and important as they are), which I had learned under Mr. William Smith in 1801, and which he has so successfully applied in the filling up of his maps of the strata in the south-east and east, and some of the middle parts of England, would fail me, in their application to the strata of Derbyshire, without taking into consideration along with them, not only the denudation, or local stripping off, of patches of strata, some of immense extent and thickness, and even more considerable than those which I had discovered to be missing* from off the Wealds of Kent, Sussex, and Surry, and had explained to you, by a rough section across this great southern denudation in 1806, and such as the valley of Ashover then appeared to present, a more perfect instance of, around us: but that previously to such denudations of the Derbyshire strata, immense dislocations or vertical derangements of very large piles of strata, separated by the fissures, called *faults* by the miners, needed also to be taken into account, for explaining the appearances of the strata and surface of the district, which I was then about to explore: *faults*, exceeding immensely in their extent and quantity of *lift* on one side (or sink on the other) any which had occurred to Mr. SMITH, in the tracing of the south-eastern strata of England, where no faults had been discovered, so considerable as to cut off entirely the connection of the strata, or in other words, to bring strata in contact on the surface, whose places in the series were too distant to be known, and readily traced in their order, in the neighbourhood. And in consequence, I judged it necessary, on my return to town, when the winter arrived, to set about the consideration of stratified masses, broken and dislocated, and then cut or denudated in all the variety of cases and degrees of each, the results of which investigation, will appear in my Report to the Board of Agriculture on Derbyshire, the first volume of which is now in the press.

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* And such as Dr. William Richardson had found to have been removed, in several places, from off the basaltic area in the counties of Derry and Antrim in Ireland, and has named *abruptions*, in his very admirable paper on this district, in the Philosophical Transactions for 1808.

With ideas thus extended, I found, on resuming my Survey in the spring of 1808, that some conclusions that I had formed, and had unfortunately committed to paper, in a sketch of a section across the county, were erroneous, and that immense *faults* occurred, in places where their existence had not been proved by miners, or generally understood, which combined with the denudations, that were so apparent in my first journey across the county in the preceding autumn, offered, as I proceeded afterwards in filling up my map, a considerably different explanation of the structure of the county, or section of its strata, from that which I had previously made, and permitted some persons to copy. The first volume of my Report to the Board of Agriculture, abovementioned, has compressed into it, all the most essential particulars of my Survey, which manuscript you did me the honour to examine, and to recommend its adoption to the Board; but as the plan of that Report did not admit of taking an extended or connected view of the great faults or dislocations of the district, I have troubled you with this Letter, in order to describe them: previous to which it may be right just to recall to your recollection, a few particulars respecting the British stratification. It is now well known to great numbers of observers, that the thick clay and other strata, on which the metropolis is situated, extend eastward through Essex, Suffolk, and Norfolk to the eastern coast, and in all their extent cover the chalk strata: that these again (the chalk) extend from the Isle of Wight to Flamborough Head, and cover other known strata, which have their regular *basset-edges*, or appearances at the surface, in continuity, to the westward of the limits of the chalk, and of each other; and thus it has been imagined by many, that the whole surface of England could be referred to, or explained by, an uninterrupted series of basset-edges of strata, dipping to the SE. and ranging in continuity from SW. to NE. in certain undulating lines, conformable to the surface, from one sea to the other, just as a certain number at the upper part of the series have been shown to do, by Mr. SMITH's manuscript maps. But, after passing the edges of the *lias* limestones and clay strata, in our progress to the westward, from any of the south-eastern and eastern parts of England, we find on the surface marks of an immense stratum of red earth or marle, which basseting from under the lias clay and sand, seems once to have extended over all the remainder of the British islands, without being now any where covered by patches of upper strata,* much beyond the continuous edge of the lias strata, abovementioned. Instead, however, of seeing the middle and all the western and northern parts of Britain covered by the same red strata, we find now, in this space, numerous local and many very large tracts of strata, surrounded by vertical and connected *faults*, and greatly lifted and tilted; from the surface of which lifted tracts, the upper red earth, and vast and very unequal thicknesses of strata, that lay in regular succession below this red earth, have been denudated, " abrupted," or carried off, leaving thus, a great variety of what have been called coal-fields, or *mineral-basins*,† in which limited tracts, great and most important series of strata, are to be seen basseting (owing to the local denudations), of which the basset-edges, or continued endings, can no where be traced in these islands, as far as I can learn. Large tracts of the intervening spaces, between these denudated mineral basins, are still occupied by the red marle, containing local strata of gypsum, rock-salt, sand, micaceous grit-stone, &c. &c. in its substance, or exposed by denudation; and in others, local strata, or nodules of great extent, or rather, perhaps, rudely crystallized masses of slate, green-stone, sienite, basalt, &c. &c. forming hills or mountains (often intersected by mineral veins) from the tops of which masses, the red marle has in most instances been denudated. It remains a task of great difficulty, yet to be accomplished, to ascertain the lower part of the British series of strata, thus only exposed to view, in local and unconnected tracts, or basins, which are in part often concealed by gravel (frequently so, near their borders), and towards which investigation, little has yet been done. It seems to me, that there are three distinct series of coal-measures, if not more, separated by thick strata of red earths, or marles, not easily distinguished from the upper one above the coal series, or that which underlays the lias strata, as abovementioned, and by thick strata of limestones; each of which red earths, probably, produce anomalous and local strata, or crystallized mountain masses, in different places, where they form the surface, and the fact of such containing no organic remains, may not have arisen from their having been formed before organized beings existed, as those contend who call them *primitive* rocks, but because the circumstances proper to crystallization, were unfitted to the propagation and life of either animals, or vegetables; and may it not be doubted, whether crystallized masses, great or small, are ever the seats of reliquia?

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* Gravels, peat, &c. not being included in this term.

† Of which a fine instance is described by Mr. EDWARD MARTIN, in the Philosophical Transactions for 1808, and of which the Forest of Dean presents a smaller, but similar instance.

The northern part of Derbyshire, and the adjoining parts of the surrounding counties, present a denudated tract, and partake of this uncertainty, as to what place in the lower part of the British series of strata, its strata should be referred: from many circumstances, I am inclined to consider the coal-field of Derbyshire, Nottinghamshire, and Yorkshire, underlaying the yellow-lime rock, as lower in the series than any others of the coal-measures alluded to above, and that the fourth limestone rock, which extends from Castleton in Derbyshire, southward to Weaver Hill, near Wooton and Ramsor in Staffordshire, is the very lowest which is known in Britain, and which may account for the circumstance, that the mineral veins and the strata in which they occur in Derbyshire, present some phenomena, which are said to occur no where else.

I shall proceed now to describe the circumstances, under which this great elevation and denudation of part of the Derbyshire strata seems to have happened, which is, by a series of three or four separately lifted tracts, one within the other, as represented in the small sketch map annexed. The outer or least lifted of these tracts is bounded on the south by a fault, that I have distinguished by a full line, where ascertained, and by slight dots where only inferred, and denominated it the great Derbyshire fault, which is perfectly defined from near Nottingham across Derbyshire, to the north side of Stone in Staffordshire (except in a few places where gravel covers it), by having red marle, lying nearly horizontal, on all its south side, and different strata on its north side, as will be mentioned further on: the eastern fault or side of this first raised tract is not visible within the limits of my Survey, like the southern, on account of the vast accumulation of quartz gravel in Sherwood Forest, and the peaty alluvia north of it: but it seems probable to me, that its range is from about the town of Nottingham, east of Mansfield, east of Worksop near Bawtry, west of Thorne in Yorkshire, and how much further north this fault proceeds, before it turns to the west, I am unable to state from my own observations; but from the correspondence of my friend WILLIAM SMITHSON, Esq. of Heath Hall near Wakefield, a very able observer, I conclude, that the boundary fault on the north of the outer lifted tract, ranging not far from the lower part of the course of the Wharf river, suddenly cuts off, or terminates the great Derbyshire and Yorkshire coal-field to the north, and continues S. of Otley and Keighley * near Colne in Lancashire and Clitheroe, bounding still the coal-field of Lancashire to the north. I am not sufficiently acquainted with the Lancashire strata to hazard a conjecture, as to where this fault turns (or branches perhaps) towards the southward again; but on the west it probably passes not far from Manchester, Stockport in Cheshire, Macclesfield, Congleton, Church-Lawton Salt-works, and joins the great Derbyshire fault, or southern boundary of this very large raised tract, somewhere to the NW. of Stone in Staffordshire, as I judge, from the information which I have received, of the red marle occupying the surface withoutside this raised tract to the westward, in Cheshire and Staffordshire,

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* It seems probable, from the accounts which I have received from Mr. SMITHSON, of the many small coal-basins, or swilleys, as they are called, which occur in the space between Keighley, Hawes, and Richmond, viz. on the N. side of Keighley; on Fountain Fell in Craven; Thorpe Fell near Burnsall, Threshfield near Linton, and Anter-Heights near Kettlewell, on the Wharf river; Netherdale Forest near Middlesmoor, on the Nidd river; Slapestones near Hawes, West-Scrafton SW. Leyburn NW. and Braithwait-Bank near to Middleham on the Yore river; Hudswell Moor SW. of Richmond on the Swale river, &c. that all these belong to the lower or calcareous part of the Newcastle coal series, as exhibited in Mr. WESTGARTH FORSTER'S "Treatise on a Section of Strata," lately published, wherein near 4100 feet thick of strata are described in order.

as well as south of it in Staffordshire, Derbyshire, and Nottinghamshire, as abovementioned, and on the east of it from Nottingham to Thorne in Yorkshire, and perhaps further northward.

This border, or plain of red marle, has the tract within it so raised, that the yellow, or magnesian lime rock, probably abuts against the marle at the surface of the strata on the east side, under the gravel, &c. from near Nottingham to near Wetherby in Yorkshire. From Lenton E. of Nottingham, to Allestry N. of Derby, the upper parts of the coal-measures in the first raised tract, abut on the red marle: here another great fault, called the zig-zag fault, intersects the boundary fault; from Allestry to the SE. corner of the Weaver Hills near Wooton in Staffordshire, the second inner tract, with a vastly greater rise than the first, abuts on this southern fault, so as to bring the great limestone-shale (which underlays all the coal-measures) against the red marle on the surface; at this SE. corner of the Weaver Hills, another great fault (called the great limestone fault) intersects the southern boundary (or great Derbyshire) fault of the raised tract; and from this place to the SW. corner of the Weaver Hills near Ramsor, a third inner tract, with four hundred yards or more of perpendicular rise, in addition to the last, occasions the fourth, or lowest limestone rock, to abut against, and even make a high hill above the red marle at the foot of it, on the other side of the great Derbyshire fault; which here occasions a sudden derangement of the strata (and a corresponding denudation of the large tract of country to the northward has taken place), far exceeding any thing which has hitherto been mentioned by authors, or conceived probably by any one.

At the SW. corner of the Weaver Hills abovementioned, the great limestone fault again leaves the south boundary, or great Derbyshire fault, and proceeds northward, after which a corner of the second interior raised tract again presents itself, and the limestone-shale again abuts on the marle, as we pursue the great Derbyshire fault to the westward, owing to the rise being less here by four or five hundred yards, than it was in the third interior tract; but as we proceed southwestward, owing to the dip of the measures on the N. side of the great Derbyshire fault towards the west, the first grit, the first coal-shale, and the second grit rock successively abut against the marle, before the gravel covering commences, east and south of Cheadle, which prevented my tracing this fault any further, within the limits of my Survey.

It seems probable, however, that somewhere SW. of Cheadle in Staffordshire, a branch sets off from the great Derbyshire fault, or southern boundary of the lifted tracts, and proceeds northward, near to Endon and Bosley in Cheshire; the triangular tract beyond which, to the westward, shewn in the map, forming the pottery coal-field, is much less raised, perhaps, than any of the other tracts which have been here mentioned.

If we return to Allestry N. of Derby abovementioned, and trace the zig-zag fault, through Little Eaton, West Hallam, and Ilkeston in Derbyshire, Awsworth, Greasley, Annesley, Kirkby, and Dirty-Hucknal in Nottinghamshire, Alt-Hucknal, Bolsover, Clown, and Barlborough in Derbyshire, Harthill, South-Anston, North-Anston, Dinnington, &c. in Yorkshire, we shall find coal-measures on both sides of it, through its whole length, except in two or three instances, where the yellow lime strata at the top of these coal-measures abut against it for short distances, and between Allestry and Little Eaton, where the limestone-shale below these coal-measures abuts on its W.; but the rise is very considerable and unequal on the N. and W. sides, through its whole length, compared with the other sides, as I have particularly shewn in my Report to the Board, and pointed out the great difficulties which this zig-zag fault has presented, to the right understanding the entire of the great Derbyshire and Yorkshire coal-field, by the many very intelligent and able colliers who are found in it.

The first, or outer raised tract, thus bounded by faults (except, perhaps on the N. where my Survey has not extended), shews no very rapid dips or inclinations of the strata, except in very limited spots, and presents on the surface either the yellow lime rock, or the parts of the coal-measures not very far beneath that rock, compared with the whole thickness of these coal-measures.

The great limestone fault (which has been mentioned above) commences in the town of Cromford in Derbyshire, in the first or upper limestone rock, and proceeds through Middleton, Wirksworth, Hopton, Carsington, Ballidon, Parwich, Newton-Grange, and Thorpe in Derbyshire, Ilam, Blore, and Thornwood in Staffordshire, and joins the great Derbyshire fault near Wooton (as abovementioned), with which it coincides along the S. end of the Weaver Hills to near Ramsor, where it again leaves this fault and proceeds near Caldon, Water-Houses, Water-fall, Grindon, Wetton Mill, S. end of Ecton Hill, near Gateham and Narrowdale in Staffordshire, Wolfscote, Beresford, Hartington, Ludwell, Pilsbury, Crowdycote, Dowall, Booth, Thirkelow, Edge-end, Buxton Baths, Black-edge, Dove-hole, crosses to the W. side, and again to the E. side of the *Grand Ridge** of the island, passes near Sparrow-Pit, Perry-foot, Odin-Mine, Lane-head, Castleton Town, Pindale, Edingtree, Bradwell, Hazlebadge, Quarters-house, and Windmill-houses, and terminates in the first lime rock between Wardlow-Mires and Litton in Derbyshire. If now a line be traced on the same first lime rock, through Wardlow, W. of Little Longsdon, W. of Ashford, through Sheldon, Callenge Low, Middleton by Yolgrave, S. of Gratton, Elton, Winster, Wensley, and Snitterton, W. of Matlock Church, Starkholmes, and Willersley Castle to Cromford Town, shewn by very fine dots in the map, this line on the first limestone, may be considered as a kind of hinge, or joint, on which the second inner raised tract, and the third inner raised tract have turned a little, and altered their inclinations with respect to each other and the surrounding tracts (without any vertical derangement at this hinge), so that the great limestone fault above described, from Middleton by Wirksworth, round to the westward through Staffordshire, as above, to Quarters-house near great Hucklow (with the exception of the short distance between Wooton and Ramsor, and some other trifling ones) has the limestone-shale (or the shale-limestone, &c. belonging to it) on its outside on the surface of the second inner raised tract, for more than fifty miles; but on its other side, owing to the great tilt or rise of the western side of the third inner raised tract, if we begin in Cromford, and pursue the course of the great limestone fault, up Bonsal-Dale, we have at first the first lime on its right or N. side (as well as on its S. side), then the first toadstone, next the second limestone, then the second toadstone, and after its turning to pass through Middleton by Wirksworth, the third limestone also abuts against it, and continues so to do, till the third toadstone appears against it at Hopton, and then the fourth limestone, or lowest known rock of the district, abuts against it all the way round, through Staffordshire to Castleton (with the exception of the hummocks of third limestone at Buxton and at Barmoor in Peak Forest, and a few sunk gulfs of shale) through a length of more than forty-five miles. At the SE. end of Castleton Town, the third toadstone abuts again on the right or S. side of this great limestone fault, and from thence to the SW. side of the Windmill-Houses, the third limestone abuts against it, then the second toadstone, the second limestone, the first toadstone, and at length the first limestone, before the fault terminates or becomes too inconsiderable to be readily traced, owing to its no longer deranging the order of the strata on the surface, but has the first limestone on both its sides; and thus it happens, that the third inner raised tract, or mineral-field, consisting of the four limestone rocks and three interposed toadstones, and containing about 105,000 acres, has about 51,500 acres of these on its eastern side, occupied by the three upper limestones and the toadstones, and the remaining 53,500 acres in Derbyshire and Staffordshire is occupied by the fourth, or lowest limestone rock, in which only a few mineral veins occur among its numerous open fissures and caverns.

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* See that article in Dr. REES's Cyclopœdia lately published.

In the sketch map which accompanies this, I have shewn a smaller, or fourth inner raised tract, on which Bakewell is situated, in great part surrounded by a fault, which I have thence denominated the great Bakewell fault: I have selected this tract, among other small local raised ones, on account of its approaching in shape and position to the others which surround it, and because it explains several curious appearances of the strata in these parts, which have been, and are still likely to be greatly misrepresented.

The fault to which I allude, may be said to commence in the limestone-shale on the E. side of Beeley, to pass on the S. side of the village, across the Derwent and on the S. of Haddon-Hall, continuing across the pastures to the Lathkil river about a quarter of a mile above Alport, then turns S. past the W. end of Alport to the upper mill, thence WSW. for about one mile, where this fault again turns to the N. and crosses the Bradford river, and proceeding across the Meadow-place Liberty, crosses Robinstye Mine and the Lathkil river above Over-Haddon mill; bears then a little to the east of the north, and crosses the new Bakewell and Buxton road at the rise of the hill, passes the N. end of Bird's-head Mine, crosses the Wye river about a quarter of a mile above Bakewell Cotton Mill, proceeds near to Rowdale, turns E. towards Nether Burchill, then NE. following nearly the course of the brook, it continues the same direction until about half a mile E. of Hassop, where it turns to the E. crosses the Derwent again half a mile above Baslow, and terminates in the limestone-shale in Barbrook Dale, as it began. The western side of this fourth inner tract being most raised (similar to the third tract) occasions the great elevation of the shale, and its freestone in the hills E. of Bakewell and NE. of Haddon-Hall; the sudden appearance of the limestone, on which Haddon-Hall stands, and in the quarry SSW. of it, on the W. of the road, and thence across the pastures to the Lathkil river; the sudden elevation of the limestone knowls SW. of Yolgrave, near the shale; and by this same lift, it happens, that the vale of the Bradford suddenly cuts through the first lime rock. as soon as it has crossed this fault, and shews the first toadstone to a considerable height up each side of the valley, but which declines with the dip of the measures in this tract, until the Bradford again gets upon the first toadstone, and then on the first limestone. In like manner, the greater rise of the measures at this fault, on the SW. of Over-Haddon village occasions the valley of the Lathkil river, which till then had been excavated in the first lime rock, to enter abruptly so deep into the first toadstone, as to lay bare a patch of the second limestone under it in the river, both of which however descend again below the bed of the river, before we get down to the crossing of the Ashburne turnpike road.

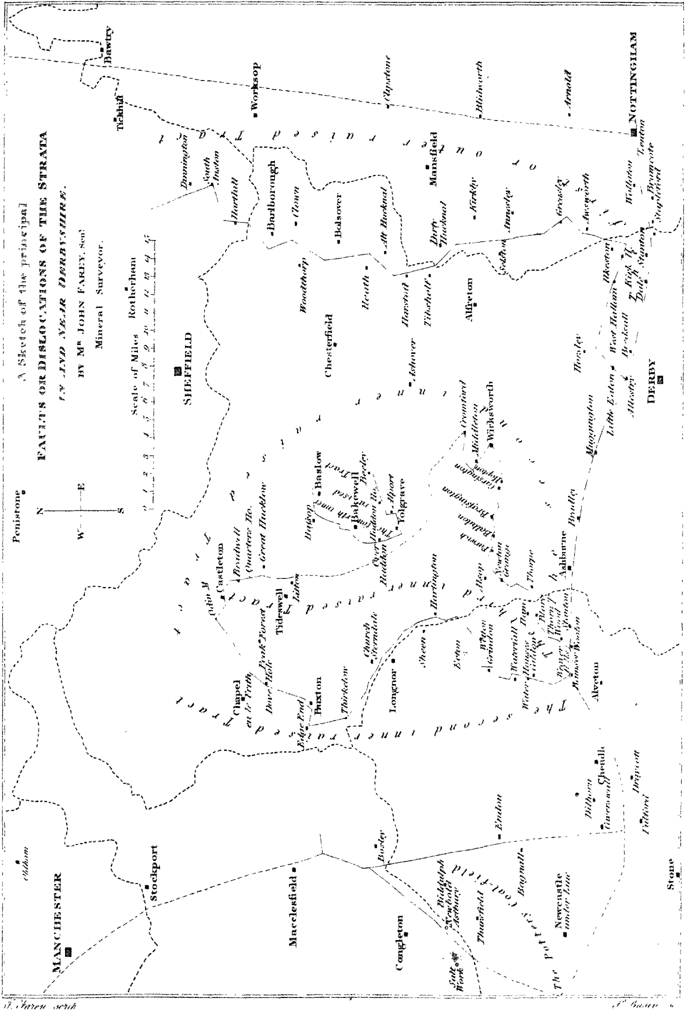
This fault also occasions the sudden appearance of shale-limestone on the surface NW. of it, opposite to first limestone on the other side in Bakewell Fields, and of the first toadstone on the NW. of Bakewell Cotton-Mill, almost excavated through by the vale of the Wye river, where it abuts against shale or shale-limestone at the northern end of this noted patch of toadstone, the situation and circumstances of which, when compared with those of the other two patches, at the edge of this same raised tract, as above, will be divested of much of that singularity which has been ascribed to it; for we see, that each of the three rivers, which pass on to this fourth inner raised tract, have their excavations cut through the first limestone, so as to expose the first toadstone for some distance, until the more rapid descents of the measures than of the vales, occasion them again to dip and disappear in the bottoms of each of these vales.

I am, Sir,

your obliged and very humble servant,

J. FAREY, SEN.
Mineral Surveyor.

Upper Crown-street, Westminster,
January 31, 1811.



J. Farey sculp.

J. Basire sc.

XIV. *An Account of an Appendix to the small Intestines of Birds.*

By James Macartney, *Esq. F. R. S.*

Read March 21, 1811.

EVERY author who has written upon the incubation of the egg, except LEVEILLE', has admitted the existence of a direct communication between the yolk bag and the small intestine of the chick, to which the name has been sometimes given of Ductus Vitello-intestinalis. It was likewise known to STENON, NEEDHAM, and MAÎTRE JAN, that this duct remains in the form of a small cœcum during life; but I do not find that any anatomist was acquainted with the great size that this part possesses in particular species, or its internal structure and uses in the adult bird.

I was led to examine this process more attentively than it had been by others, in consequence of meeting with it in the snipe, in which bird it surpasses in length the cœca of the great intestines.

The appendix has an uniform structure in all the birds I have inspected for it. There appear to be but two tunics; the external is the continuation of the peritonæum; the internal of the villous coat of the intestine. The inner surface of the intestines exhibits different appearances, according to the species. In some it furnishes fine villous processes, in others zig-zag or waving laminæ, which produce a texture to the naked eye, like that of twilled cloth; but whatever may be the arrangement of the internal coat of the intestine, the appendix is constantly found to present a surface composed of small cells, similar to those assemblages of mucous follicles that are met with in different parts of the alimentary canal. I have not observed in any instance, that the appendix received the natural contents of the adjoining intestine, but have always found it filled with a mucous fluid, I therefore conclude that it performs the office of a mucous gland.

The magnitude and form of the appendix are not so determined as its structure. In the snipe, as already mentioned, it exceeds in length the cœca of the great intestines; its width also is equal to that portion of the gut, from which it arises; but it becomes smaller towards the extremity.

In the curlew I have found it of a very considerable size, although less in proportion to the intestine than in the snipe. It is wider in the middle than at its origin or termination.

In the woodcock it is nearly as capacious as in the curlew.

I have found it very slender, but above an inch long, in the black coot. In this bird the intestines are all long and slender, and the cœca of the great intestines singularly so.

The appendix in the swan and goose is rather larger than in birds generally, and ends in a point.

It is remarkably small in the heron.

In a great number of the passerine and gallinaceous birds that I have examined for the purpose, I have found it very small.

It is short also in the hawk.

The greater size which this process of the intestine preserves in the snipe, curlew, and woodcock, would seem to depend upon the mode of feeding and habits of those birds;

although I do not perceive in what manner this peculiarity of structure becomes useful to them.

The appendix in most birds retains the marks of its origin; thus the remnant of the yolk bag is commonly found attached to its free extremity, with which it still preserves a communication. This occurs particularly in the passerine tribe, and amongst the accipitres. In the nightingale the duct is so short, that it is scarcely visible; but the yolk bag remains during life about the size of a small pea, adhering to the intestine with which it communicates.

The preceding facts furnish a curious example of the œconomy of nature, in adapting an organ of fœtal life to the exercise of a particular function in the full grown bird.

They likewise afford a proof that the yolk bag and intestine not only have communicated, but that they were originally continuous structures, a fact which has been lately denied by LEVEILLE′.

At a future period, I hope to lay before the Society, some observations upon the functions of the different parts of the egg during incubation, from which it will be found, that a communication between the yolk bag and intestines exists as a matter of necessity.

EXPLANATION OF THE FIGURES.

Fig. 1. Represents a considerable portion of the intestinal canal of the snipe, in order to show the relative size of the appendix in that bird.

a. Indicates the small intestine.

b. The great intestine, or rectum.

c. Is the appendix, on which may be seen, even upon the external surface, the marks of the cellular structure. At the extremity of the appendix, the remnant of the yolk bag may be perceived.

d. Shows the two cœca of the great intestine, which are shorter in this bird than the appendix.

Fig. 2. Is a short portion of the small intestine, with the appendix, in the curlew.

a. Is the intestine laid open, to exhibit the plicated, or twilled appearance of the villous coat which is a common structure in birds.

b. The appendix opened also, to expose the cellular surface internally.

c. Is the remnant of the yolk.

N. B. This figure represents the parts perhaps a little larger than natural, owing to the drawing being made from a preparation in spirits; but the proportion between the appendix and intestine are nevertheless correct.

Fig. 1.

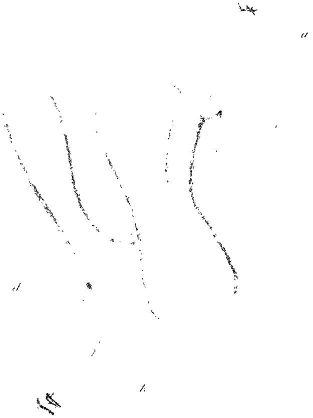
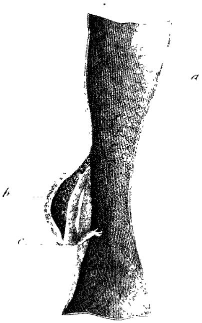


Fig. 2.



XV. *An Account of a vegetable Wax from Brazil. By* William Thomas Brande, *Esq. F. R. S.*

Read May 9, 1811.

§. I.

THE vegetable wax described in this Paper, was given to the President by Lord GRENVILLE, with a wish, on the part of his Lordship, that its properties should be investigated, in the hope that it might prove an useful substitute for bees wax, and constitute, in due time, a new article of commerce between the Brazils and this country.

It was transmitted to Lord GRENVILLE from Rio de Janeiro, by the Comte DE GALVEAS, as a new article lately brought to that city, from the northernmost parts of the Brazilian dominions, the Capiteneas of Rio Grande and Seara, between the latitudes of three and seven degrees north: it is said to be the production of a tree of slow growth, called by the natives *Carnauba*, which also produces a gum used as food for men, and another substance employed for fattening poultry.

When the Comte wrote to Lord GRENVILLE in July last, orders had been sent to the governors of the districts where it grows, requiring them to report more particularly, on the nature and qualities of this interesting tree; we may therefore hope that information will soon be obtained, whether the article can be procured in abundance, and at a reasonable price, in which case it will become a valuable addition to the comforts of mankind, by reducing the price and improving the quality of candles, flambeaux, &c.

The article, in the state in which it was sent, resembles much that described by Humboldt, as the produce of the Ceroxylon Andicola,* but it is not likely to be the same, as Humboldt's wax is collected from a stately palm tree, which grows on the high mountains, from 900 to 1450 toises above the level of the sea, and on the edge of the regions of perpetual snow. On the other hand, the Brazilian plant is described as a slow growing tree, but not as a large one, and there are no high mountains delineated in the most accurate and recent maps of the Capiteneas where it is found. But a more decisive argument against their identity, is the analysis of Vaquelin, published by Humboldt, which shews that the produce of the Ceroxylon consists of two-thirds resin and only one-third wax; but the Brazilian article is entirely wax, and affords not the smallest trace of resin. The Brazilian plant, however, was not entirely unknown to Humboldt, for it appears from his book, that M. Correa had informed him that a palm, called *Carnauba* by the natives of Brazil, produced wax from its leaves.

§. II.

1. The wax in its rough state, is in the form of a coarse pale gray powder, soft to the touch, and mixed with various impurities, consisting chiefly of fibres of the bark of the tree, which, when separated by a sieve, amount to about 40 per cent.

It has an agreeable odour, somewhat resembling new hay, but scarcely any taste.

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* Plantes Equinoctiales, p. 3.

At 206° FAHRENHEIT, it enters into perfect fusion, and in this state it may be further purified, by passing it through fine linen. By this process, it acquires a dirty green colour, and its peculiar smell becomes more evident. When cold, it is moderately hard and brittle. Its specific gravity is ,980.

2. Water exerts no action on the wax, unless boiled with it for some hours; it then acquires a slight brown tinge, and the peculiar odour of the wax.

3. Alcohol does not dissolve any portion of the wax, unless heat be applied.

Two fluid ounces of boiling alcohol, spec. grav. ,826, dissolve about ten grains of the wax, of which eight grains are deposited as the solution cools, and the remaining two grains may be afterwards precipitated by the addition of water, or may be obtained unaltered by evaporating the alcohol.

The solution of the wax in alcohol has a slightly green tinge.

4. Sulphuric ether, spec. grav. .7563, dissolves a very minute portion of the wax, at the temperature of 60°.

Two fluid ounces of boiling sulphuric ether dissolve thirty grains of the wax, of which twenty-six grains are deposited by cooling the solution, and the remaining four grains may be obtained by allowing the ether to evaporate spontaneously.

5. The fixed oils very readily dissolve the wax at the temperature of boiling water, and form with it compounds of an intermediate consistence, very analogous to those which are obtained with common bees wax.

In examining some combinations which I had made of the vegetable wax with olive oil, I was surprised to find them perfectly soluble in ether, and sparingly soluble in boiling alcohol.

As it is commonly stated that the fixed oils are insoluble in ether and in alcohol, I was led to attribute the solution of the oil, in these instances, to its being combined with the wax; but subsequent experiments, of which I shall state the general results, have shewn me that these opinions are erroneous.

Four fluid ounces of sulphuric ether, spec. grav. ,7563, dissolve a fluid ounce and a quarter of the expressed oil of almonds; of olive oil, the same quantity of the ether dissolves a fluid ounce and a half; of linseed oil, two fluid ounces and a half; and castor oil is soluble in any proportion in sulphuric ether of the above specific gravity.

The expressed oils of almonds and of olives, are very sparingly soluble in alcohol, spec. grav. ,820.

Linseed oil is more soluble than the two former. Four fluid ounces of alcohol, spec. grav. ,820, dissolve nearly one fluid drachm.

Castor oil is perfectly soluble in every proportion in alcohol, spec. grav. ,820. In alcohol of a higher specific gravity, as ,840, it is very sparingly soluble.*

As some of the difficultly soluble resins are more easily dissolved in alcohol, to which a small proportion of camphor has been added, I endeavoured to ascertain whether the fixed oils were rendered more soluble by the same means, but found that this was not the case, excepting with regard to castor oil, which although very sparingly dissolved by alcohol of a spec.

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* The solubility of castor oil in alcohol was mentioned to me some months ago by Dr. WOLLASTON, who also informed me, that it had on that account been employed to adulterate certain essential oils of high value, especially the oil of cloves.

grav. above ,840°, becomes abundantly soluble, by the addition of one part of camphor, to eight parts of the alcohol.

Boiling alcohol, spec. grav. ,840, takes up a considerable portion of castor oil and of linseed oil; it also dissolves a small quantity of the oils of almonds and of olives; but they are copiously deposited during the cooling of the alcohol, and only a small portion retained in permanent solution.

When water is added to any of these solutions of the fixed oils in ether, and in alcohol, a milky mixture is formed, and the oil gradually separates upon the surface, without having undergone any apparent alteration.

6. One hundred grains of the wax were boiled for half an hour in a solution of caustic potash, spec. grav. 1090. The solution acquired a pale rose colour, but appeared to exert no further action on the wax, which after having been washed with warm water, retained its fusibility and other properties. No combination therefore, similar to a soap, was produced, nor was any precipitate occasioned by the addition of acids to the rose coloured alkaline solution.

7. The effects produced by boiling the wax in solutions of pure soda, and of the subcarbonates of soda and of potash, were analogous to those of the caustic potash.

8. Solutions of pure and of carbonated ammonia exert scarcely any action on the wax.

9. When the wax is boiled in nitric acid, spec. grav. 1,45. there is some escape of nitrous gas, and the colour of the wax is gradually changed to a deep yellow.

When the wax is removed from the acid, and washed with hot water, it is found to have become more brittle and hard, but it still retains much of its peculiar odour.

In this state it remains insoluble in the alkalies, but they now change its colour to a very bright brown, which is destroyed by washing with dilute muriatic acid, and its original yellow colour restored.

Neither the fusibility, nor the inflammability of the wax, are impaired by this process.

Nitric acid, diluted with eight parts of water, produces the same change in the colour of the wax as the concentrated acid.

Having been unsuccessful in my attempts to bleach the wax in its original state, I made some experiments to ascertain whether its colour could be more easily destroyed, after it had been acted upon by nitric acid, and found, that by exposing it spread upon glass to the action of light, it became in the course of three weeks of a pale straw colour, and on the surface nearly white. The same change was produced, by steeping the wax in thin plates, in an aqueous solution of oxymuriatic gas, but I have not hitherto succeeded in rendering it perfectly white.

10. Muriatic acid has little action on the wax: when boiled upon it for some hours, it destroys much of its colour.

11. Sulphuric acid changes the colour of the wax to a pale brown, and when water is added, it becomes of a deep rose colour; the inflammability and the fusibility of the wax are slightly impaired by this process.

When heat is applied, the wax is decomposed with the usual phenomena, sulphurous acid is developed, and charcoal deposited.

12. Acetic acid has very little action on the wax, when cold.

When the wax is boiled in this acid, a minute portion is dissolved, and again deposited as the solution cools. By long continued boiling in acetic acid, the wax is rendered nearly white; but when it is afterwards washed with water, and fused, it resumes its former colour.

13. When the wax is fused in oxymuriatic gas, it is rapidly decomposed, and parting with hydrogen and oxygen, muriatic acid and water are formed, and charcoal is deposited.

14. The results of the destructive distillation of the vegetable wax, are very analogous to those of bees wax.

An acid liquor, mixed with a volatile oil; are the first products; these are succeeded by a large proportion of a butyraceous oil, and a very small quantity of charcoal affording traces of lime, remains in the retort. During the process, a little carbureted hydrogene gas is given off.

I have not considered it necessary to dwell upon the relative proportions of these different products, as they will necessarily vary according to the rapidity with which the distillation is conducted.

§. III.

From the preceding detail of experiments, it appears, that although the South American vegetable wax possesses the characteristic properties of bees wax, it differs from that substance in many of its chemical habitudes; it also differs from the other varieties of wax, namely, the wax of the myrica cerifera,* of lac,† and of white lac.‡

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* Vide Dr. BOSTOCK's Experiments on the Wax of the Myrica cerifera, in NICHOLSON's Journal for March 1803.

† Vide Analytical Experiments and Observations on Lac, by CHARLES HATCHETT, Esq. F. R. S. in the Philosophical Transactions for 1804.

‡ Vide Observations and Experiments on a Wax like Substance from Madras, by GEORGE PEARSON, M. D. F. R. S. in the Philosophical Transactions for 1794.

The attempts which I have made to bleach the wax, have been conducted on a small scale; but from the experiments related, it appears that after the colour has been changed by the action of very dilute nitric acid, it may be rendered nearly white by the usual means. I have not had sufficient time to ascertain whether the wax can be more effectually bleached by long continued exposure, nor have I had an opportunity of submitting it to the processes employed by the bleachers of bees wax.

Perhaps the most important part of the present inquiry, is that which relates to the combustion of the vegetable wax, in the form of candles.

The trials which have been made to ascertain its fitness for this purpose, are extremely satisfactory, and when the wick is properly proportioned to the size of the candle, the combustion is as perfect and uniform, as that of common bees wax.

The addition of from one-eighth to one-tenth part of tallow, is sufficient to obviate the brittleness of the wax in its pure state, without giving it any unpleasant smell, or materially impairing the brilliancy of its flame. A mixture of three parts of the vegetable wax, with one part of bees wax, also makes very excellent candles.

XVI. *Astronomical Observations relating to the Construction of the Heavens, arranged for the Purpose of a critical Examination, the Result of which appears to throw some new Light upon the Organization of the celestial Bodies. By* William Herschel, *LL.D. F. R. S.*

Read June 20, 1811.

A KNOWLEDGE of the construction of the heavens has always been the ultimate object of my observations, and having been many years engaged in applying my forty, twenty, and large ten feet telescopes, on account of their great space-penetrating power to review the most interesting objects discovered in my sweeps, as well as those which had before been communicated to the public in the *Connoissance des Temps*, for 1784, I find that by arranging these objects in a certain successive regular order, they may be viewed in a new light, and, if I am not mistaken, an examination of them will lead to consequences which cannot be indifferent to an inquiring mind.

If it should be remarked that in this new arrangement I am not entirely consistent with what I have already in former papers said on the nature of some objects that have come under my observation, I must freely confess that by continuing my sweeps of the heavens my opinion of the arrangement of the stars and their magnitudes, and of some other particulars, has undergone a gradual change; and indeed when the novelty of the subject is considered, we cannot be surprised that many things formerly taken for granted, should on examination prove to be different from what they were generally, but incautiously, supposed to be.

For instance, an equal scattering of the stars may be admitted in certain calculations; but when we examine the milky way, or the closely compressed clusters of stars, of which my catalogues have recorded so many instances, this supposed equality of scattering must be given up. We may also have surmised nebulæ to be no other than clusters of stars disguised by their very great distance, but a longer experience and better acquaintance with the nature of nebulæ, will not allow a general admission of such a principle, although undoubtedly a cluster of stars may assume a nebulous appearance when it is too remote for us to discern the stars of which it is composed.

Impressed with an idea that nebulæ properly speaking were clusters of stars, I used to call the nebulosity of which some were composed, when it was of a certain appearance, *resolvable;* but when I perceived that additional light, so far from resolving these nebulæ into stars, seemed to prove that their nebulosity was not different from what I had called milky, this conception was set aside as erroneous. In consequence of this, such nebulæ as afterwards were suspected to consist of stars, or in which a few might be seen, were called *easily resolvable;* but even this expression must be received with caution, because an object may not only contain stars, but also nebulosity not composed of them.

It will be necessary to explain the spirit of the method of arranging the observed astronomical objects under consideration in such a manner, that one shall assist us to understand the nature and construction of the other. This end I propose to obtain by assorting them into as many classes as will be required to produce the most gradual affinity between the individuals contained in any one class with those contained in that which precedes and that which follows it: and it will certainly contribute to the perfection of this method, if this connection between the various classes can be made to appear so clearly as not to admit of a doubt. This consideration will be a sufficient apology for the great number of assortments into which I have thrown the objects under consideration; and it will be found that those contained in one article, are so closely allied to those in the next, that there is perhaps not so much difference between them, if I may use the comparison, as there would be in an annual description of the human figure, were it given from the birth of a child till he comes to be a man in his prime.

The similarity of the objects contained in each class will seldom require the description of more than one of them, and for this purpose, out of the number referred to, the selected one will be that which has been most circumstantially observed; however, those who wish either to review any other of the objects, or to read a short description of them, will find their place in the heavens, or the account of their appearance either in the catalogues I have given of them in the Philos. Trans. or in the *Connoissance des Temps* for 1784, to which in every article proper references will be given for the objects under consideration.

If the description I give should sometimes differ a little from that which belongs to some number referred to, it must be remembered that objects which had been observed many times, could not be so particularly and comprehensively detailed in the confined space of the catalogues as I now may describe them; additional observations have also now and then given me a better view of the objects than I had before. This remark will always apply to the numbers which refer to the *Connoissance des Temps;* for the nebulæ and clusters of stars are there so imperfectly described, that my own observation of them with large instruments may well be supposed to differ entirely from what is said of them. But if any astronomer should review them, with such high space-penetrating-powers, as are absolutely required, it will be found that I have classed them very properly.

It will be necessary to mention that the nebulous delineations in the figures are not intended to represent any of the individuals of the objects which are described otherwise than in the circumstances which are common to the nebulæ of each assortment: the irregularity of a figure, for instance, must stand for every other irregularity; and the delineated size for every other size. It will however be seen, that in the figure referred to there is a sufficient resemblance to the described nebula to show the essential features of shape and brightness then under consideration.

1. *Of extensive diffused Nebulosity.*

The first article of my series will begin with extensive diffused nebulosity, which is a phenomenon that hitherto has not been much noticed, and can indeed only be perceived by instruments that collect a great quantity of light. Its existence, when some part of it is pointed out by objects that are within the reach of common telescopes, has nevertheless obtruded itself already on the knowledge of astronomers, as will be seen in my third article.

The widely diffused nebulosity under consideration has already been partially mentioned in my catalogues.*

The description of the object I shall select is of No. 14 in the 5th class, and is as follows: " Extremely faint branching " nebulosity; its whitishness is entirely of the milky kind, and " it is brighter in three or four places than in the rest; the " stars of the milky way are scattered over it in the same " manner as over the rest of the heavens. Its extent in the " parallel is nearly 1½ degree, and in the meridional direction " about 52 minutes. The following part of it is divided into " several streams and windings, which after separating, meet " each other again towards the south." See figure 1.

This account, which agrees with what will be found in all the other numbers referred to, with regard to the subject under consideration, namely, a diffused milky nebulosity, will give us already some idea of its great abundance in the heavens; my next article however will far extend our conception of its quantity.

2. *Observations of Nebulosities that have not been published before.*

It may be easily supposed that in my sweeps of the heavens I was not inattentive to extensive diffusions of nebulosity, which occasionally fell under my observation. They can only be seen when the air is perfectly clear, and when the observer has been in the dark long enough for the eye to recover from the impression of having been in the light.

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* See Phil. Trans. for 1786, page 471; for 1789, page 226; and for 1802, page 503. The following ten nebulosities are in the Vth class, No. 13, 14, 15, 17, 28, 30, 31, 33, 34, 38.

I have collected fifty-two such observations into a table, and have arranged them in the order of right ascension. In the first column they are numbered; in the second and third columns are the right ascension and north polar distance of a place which is the central point of a parallelogram comprehending the space which the nebulosity was observed to fill. They are calculated for the year 1800.

The length and breadth of the parallelograms are set down in the 4th and 5th columns in degrees and minutes of a great circle. The time taken up in the transit of each parallelogram having been properly reduced to space by the polar distance given in the 3d column, in order to make it agree with the space contained in the breadth of the zone described by the telescope; the dimensions of the former space therefore is in the parallel, and that of the latter in the meridian. My field of view, being fifteen minutes in diameter, its extent has been properly considered in the assigned dimensions of the parallelograms. It is however evident that the limits of the sweeping zone leave the extent of the nebulosity in the meridian unascertained. The beginning of it is equally uncertain, since the nebulous state of the heavens could only be noticed when its appearance became remarkable enough to attract attention. The ending is always left undetermined; for, as the right ascension was only taken once, I have allowed but a single minute of time for the extent of the nebulosity in that direction, except where the time was repeatedly taken with a view to ascertain how far it went in the parallel; or when the circumstances of its brightness pointed out a longer duration.

The sixth column of the table contains the size of the observed nebulosity reduced to square degrees and decimals, computed from the two preceding columns; and in the last I have given the account of these nebulosities as recorded in my sweeps at the time they were made; namely within a period of nineteen years, beginning in 1783 and ending in 1802.

When this account says *affected*, it is intended to mean that the ground upon which, or through which we see, or may see stars, is affected with nebulosity.

Table of extensive diffused Nebulosity.

No.	R. A.	P. D.	Paral.	Merid.	Size.	Account of the Nebulosity.
	h ′ ″	° ′	° ′	° ′	Deg.	
1	0 5 2	81 7	1 44	1 55	3,3	Much affected with nebulosity.
2	0 12 31	85 34	3 0	2 34	7,7	Much affected.
3	0 17 17	61 24	0 41	2 40	1,8	Affected.
4	0 20 31	86 34	1 30	2 34	3,6	Much affected.
5	0 25 5	67 8	0 29	2 34	1,2	Much affected.
6	0 31 22	90 4	2 30	2 19	5,7	Appeared to be affected with very faint nebulosity.
7	0 32 54	49 23	1 33	3 1	4,7	Affected with nebulosity.
8	0 34 21	51 17	1 17	2 49	3,6	Unequally affected.
9	0 36 13	47 3	2 37	3 18	8,6	Suspected faint nebulosity.
10	0 43 32	46 58	0 26	3 18	1,4	Suspected faint nebulosity.
11	1 35 32	60 42	0 28	2 40	1,3	Suspected to be tinged with milky nebulosity.
12	2 22 19	71 27	0 29	2 29	1,2	Much affected with nebulosity.
13	3 56 14	65 6	0 29	2 27	1,7	Much affected.
14	4 17 21	55 7	1 4	2 38	2,8	Suspected pretty strong nebulosity.
15	4 18 21	55 6	1 53	2 38	5,0	Suspected nebulosity
16	4 21 35	97 44	0 30	2 15	1,1	Strong milky nebulosity.
17	4 23 14	69 23	0 29	2 36	1,3	Much affected.
18	4 38 17	69 23	0 29	2 36	1,3	Much affected.
19	4 46 17	63 25	1 46	2 31	4,4	Strong suspicion of very faint milky nebulosity.
20	5 9 44	65 6	1 23	2 27	3,4	Very much affected.
21	5 13 14	65 6	0 29	2 27	1,7	Affected.

No.	R. A.	P. D.	Paral.	Merid.	Size.	Account of the Nebulosity.
	h ′ ″	° ′	° ′	° ′	Deg.	
22	5 23 59	97 1	2 31	2 31	6,3	Affected with milky nebulosity.
23	5 25 16	92 48	0 30	2 40	1,3	Affected.
24	5 27 2	94 23	1 48	2 32	4,6	Visible and unequally bright nebulosity. I am pretty sure this joins to the great nebula in Orion.
25	5 30 40	92 35	2 45	2 33	7,0	Diffused milky nebulosity.
26	5 31 58	97 1	1 56	2 31	4,9	A pretty strong suspicion of nebulosity.
27	5 38 5	88 55	1 6	2 37	2,9	Affected with milky nebulosity.
28	5 55 55	86 17	0 30	2 34	1,3	Much affected.
29	5 56 36	110 28	1 48	2 48	5,0	Affected.
30	6 33 7	48 39	0 26	3 4	1,3	Affected.
31	9 22 56	108 3	0 29	2 30	1,2	Affected.
32	9 27 19	18 21	0 24	4 4	1,6	Much affecred with very faint whitish nebulosity.
33	10 6 56	98 33	3 58	2 17	9,1	Very faint whitish nebulosity.
34	10 16 1	37 58	0 24	4 9	1,7	Much affected.
35	10 34 29	26 44	0 29	3 15	1,6	Affected with very faint nebulosity
36	10 58 24	26 44	0 42	3 15	2,3	Affected.
37	11 56 59	58 50	0 41	2 54	2,0	Affected with whitish nebulosity.
38	12 7 34	58 50	0 41	2 54	2,0	Affected with whitish nebulosity.
39	13 7 33	55 20	0 27	2 17	1,0	Much affected.
40	13 58 0	55 20	0 42	2 17	1,6	Very much affected; and many faint nebulæ suspected.
41	15 5 7	70 40	1 52	2 31	4,7	Affected with very faint nebulosity.
42	20 58 20	92 17	1 45	2 21	4,1	Much affected with whitish nebulosity.
43	20 48 50	73 38	0 29	2 52	1,4	A good deal affected.
44	20 51 4	46 51	0 59	2 53	2,8	Faint milky nebulosity scattered over this space, in some places pretty bright.
45	20 52 28	91 57	0 49	0 56	0,8	Much affected with whitish nebulosity.
46	20 53 31	47 7	1 8	3 18	3,7	Suspected nebulosity joining to plainly visible diffused nebulosity.
47	21 0 26	76 3	0 44	2 46	2,0	Affected.
48	21 29 27	80 8	0 30	2 15	1,1	Much affected.
49	21 42 16	68 57	0 29	2 36	1,2	Affected.
50	22 52 36	64 47	0 29	2 47	1,3	Much affected.
51	22 53 6	64 47	0 42	2 47	1,9	Affected.
52	22 55 29	61 15	0 28	2 37	1,2	A little affected.

In looking over this table, it may be noticed that I have inserted several nebulosities that were only suspected. Had I been less scrupulous at the time of observation the word suspected would generally have been omitted; for with this nebulosity, as well as with the great number of nebulæ that in my catalogues are marked suspected, I have almost without exception found, in a second review, that the entertained suspicion was either fully confirmed, or that, without having had any previous notice of the former observation, the same suspicion was renewed when I came to the same place again.

When these observations are examined with a view to improve our knowledge of the construction of the heavens, we see in the first place that extensive diffused nebulosity is exceedingly great indeed; for, the account of it, as stated in the table, is 151,7 square degrees; but this, it must be remembered, gives us by no means the real limits of it, neither in the parallel nor in the meridian; moreover the dimensions in the table give only its superficial extent; the depth or third dimension of it may be far beyond the reach of our telescopes; and when these considerations together are added to what has been said in the foregoing article, it will be evident that the abundance of nebulous matter diffused through such an expansion of the heavens must exceed all imagination.

By nebulous matter I mean to denote that substance, or rather those substances which give out light, whatsoever may be their nature, or of whatever different powers they may be possessed.

Another remark of equal importance arises from the consideration of the observed nebulosities. By the account of the table we find that extreme faintness is predominant in most of them; which renders it probable that our best instruments will not reach so far into the profundity of space, as to see more distant diffusions of it. In No. 44 of the table, we have an instance of faint milky nebulosity, which, though pretty bright in some places, was completely lost from faintness in others; and No. 46 confirms the same remark. It has also been already mentioned in the first article, that the nebulosity in V. 14 was brighter in three or four places than in the rest. The stars also of the milky way which were scattered over it, and were generally very small, appeared with a brilliancy that will admit of no comparison with the dimness of the brightest nebulosity. In consequence of this, we may already surmise that the range of the visibility of the nebulous matter is confined to very moderate limits.

3. *Of Nebulosities joined to Nebulæ.*

The nature of diffused nebulosity is such that we often see it joined to real nebulæ; for instances of this kind we have the fourteen following objects.*

The account of the three first nebulæ being shortened in the catalogue, I give it here more at length.

No. 81 in the first class is "A considerable bright and large "nebula. Its nebulosity is of the milky kind, and a small "part of it is considerably brighter than the rest. The greatest extent of the milkiness is preceding the bright part, and "the termination of it is imperceptible." To No. 207 should be added "It seems to join to imperceptible nebulosity on the "south preceding side;" and to No. 214, "It terminates

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* See I. 81, 207, 214. IV. 41. V. 32, 35, 37, 44, 51, 52. *Connoissance des Temps* 17, 42, 64, 78.

"abruptly to the north and is diffused to the south." See fig. 2.

No. 42 of the *Connoissance* is the great nebula in the constellation of Orion discovered by HUYGHENS. This highly interesting object engaged my attention already in the beginning of the year 1774, when viewing it with a NEWTONIAN reflector I made a drawing of it, to which I shall have occasion hereafter to refer; and having from time to time reviewed it with my large instruments, it may easily be supposed that it was the very first object to which, in February 1787, I directed my forty feet telescope. The superior light of this instrument shewed it of such a magnitude and brilliancy that, judging from these circumstances, we can hardly have a doubt of its being the nearest of all the nebulæ in the heavens, and as such will afford us many valuable informations. I shall however now only notice that I have placed it in the present order because it connects in one object the brightest and faintest of all nebulosities, and thereby enables us to draw several conclusions from its various appearance.

The first is that the extensive diffused nebulosities contained in the objects of the preceding articles are of the same nature with the nebulosity in this great nebula; for when we pursue it in its extensive course it assumes precisely the same appearance as the before-mentioned diffused nebulosities.

The second consequence we may draw from the circumstance of its containing both the brightest and faintest nebulosity joined in one object is a confirmation of an opinion already conceived in the second article, namely, that the range of the visibility of nebulous matter is what may be called very limited. The depth of the nebula may undoubtedly be exceedingly great, but when we consider that its greatest brightness does not equal that of small telescopic stars, as may be seen by comparing four of them situated within the inclosed darkness of the nebula, and several within its brightest appearance, with the intensity of the nebulous light; it cannot be expected that such nebulosities will remain visible when exceedingly farther from us than this prime nebula: the ratio of the known decrease of light will not admit of a great range of visibility within the narrow limits whereby this shining substance can affect the eye.

From this argument a secondary conclusion may be drawn, which adds to what has already been said in the foregoing article, namely, that if our best telescopes cannot be expected to reach the nebulous matter, which by analogy we may suppose to be lodged among the very small stars plainly to be seen by them; the actual quantity of its diffusion may still farther exceed even the vast abundance of it already proved to exist. A nebulous matter, diffused in such exuberance throughout the regions of space, must surely draw our attention to the purpose for which it probably may exist; and it must be the business of a critical inquirer to attend to all the appearances under which it will be exposed to his view in the following observations.

4. *Of detached Nebulosities.*

The nebulosities of the preceding articles are not restricted to an extensive diffusion; we meet with them equally in detached collections; I shall only mention the following six.*

V. 21 consists of " A broad faint nebulosity extended in the

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* See I. 92. V. 21, 26, 36, 41, 42.

"form of a parallelogram with a short ray from the preceding corner towards the south. The nebulosity is nearly of an equal brightness throughout the parallelogram, which is about 8′ long and 5 or 6′ broad, but ill defined." See fig. 3, *a*, *b*, *c*.

5. *Of milky Nebulæ.*

When detached nebulosities are small we are used to call them nebulæ, and it is already known from my catalogues that their number is very great. It will therefore be sufficient to refer only to a few, of which the nebulosity is of the milky kind.*

No. 9 in the 5th class is "A large, extended, broad, faint nebula; its nebulosity, like that of the preceding one (which is De la Caille's last but one in the Catalogue des Nebuleuses du Ciel Austral†) is of the milky kind."

The only purpose for which the nebulæ of these two classes have been placed in this connection, is to show that large detached nebulosities, whatever may be their appearance, as well as those nebulæ expressly called milky, partake of the general nature of the diffused nebulous matter, pointed out in the preceding articles.

6. *Of milky Nebulæ with Condensation.*

In looking at the beautiful nebula in Orion; to which I refer, because every common good telescope will shew it sufficiently well for the present purpose; we perceive that it is not equally bright in all its parts, but that its light is more condensed in some places than in others. The idea of condensation occurs so naturally to us when we see a gradual increase of light, that we can hardly find a more intelligible mode of expressing ourselves than by calling it condensed. The numerous instances that will be given hereafter of nebulæ that have this kind of condensation, renders it unnecessary to refer to more than the following four.*

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* See I. 204, III. 1, 116. IV. 7, 20, 30. V. 9, 25.

† See Connoissance des Temps for 1784, page 272.

The first of these, No. 11 in the first class, is "A bright "nebula of some extent, although not very large. It is of an "irregular figure, and the greatest brightness lies towards "the middle. The whitishness of this nebula is of the milky "kind." See figure 4.

By attending to the circumstances of the size and figure of this nebula, we find that we can account for its greater brightness towards the middle in the most simple manner by supposing the nebulous matter of which it is composed to fill an irregular kind of solid space, and that it is either a little deeper in the brightest place, or that the nebulosity is perhaps a little more compressed. It is not necessary for us to determine at present to which of these causes the increase of brightness may be owing; at all events it cannot be probable that the nebulous matter should have different powers of shining such as would be required independent of depth or compression.

7. *Of Nebulæ which are brighter in more than one Place.*

It is not an uncommon circumstance that the same nebula is brighter in several different places than in the rest of its compass. The following six are of this sort.†

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* See I. 11, 84. III. 457. IV. 12.

† See I. 165, 213, 261. II. 297, 406. III. 49.

No. 213 in the first class is "A very brilliant and considerably large nebula, extended in a direction from south preceding to north following. It seems to have three or four "bright nuclei." See fig. 5.

From this construction of the nebula, we may draw some additional information concerning the point which was left undetermined in my last article; for since there it was proposed as an alternative, that the nebulous matter might either be of a greater depth or more compressed in the brightest part of the nebula then under consideration, we have now an opportunity to examine the probability of each case. If here the appearance of several bright nuclei is to be explained by the depth of the nebulous matter, we must have recourse to three or four separate very slender and deep projections, all situated exactly in the line of sight; but such a very uncommon arrangement of nebulous matter cannot pretend to probability; whereas a moderate condensation, which may indeed be also accompanied with some little general swelling of the nebulous matter about the places which appear like nuclei, will satisfactorily account for their superior brightness.

The same method of reasoning may be as successfully applied to explain the number of unequally bright places in the diffused nebulosities which have been described in the 1st, 2d, and 3d articles. For instance, in the branching nebulosity V. 14, we find three or four places brighter than the rest—in the nebulosity No. 44 of the table we have places of different brightness. In the nebula of Orion, there are many parts that differ much in lustre; and in V. 37 of the same article I found, by an observation in the year 1790, the same variety of appearance. In all these cases a proportional condensation of the nebulous matter in the brighter places will sufficiently account for their different degree of shining.

This way of explaining the observed appearances being admitted, it will be proper to enter into an examination of the probable cause of the condensation of the nebulous matter. Should the necessity for such a condensing cause be thought to be admitted upon too slight an induction, a more detailed support of it will hereafter be found in the condition of such a copious collection of objects, as will establish its existence beyond all possibility of doubt.*

Instead of inquiring after the nature of the cause of the condensation of nebulous matter, it would indeed be sufficient for the present purpose to call it merely a condensing principle; but since we are already acquainted with the centripetal force of attraction which gives a globular figure to planets, keeps them from flying out of their orbits in tangents, and makes one star revolve around another, why should we not look up to the universal gravitation of matter as the cause of every condensation, accumulation, compression, and concentration of the nebulous matter? Facts are not wanting to prove that such a power has been exerted; and as I shall point out a series of phenomena in the heavens where astronomers may read in legible characters the manifest vestiges of such an exertion, I need not hesitate to proceed in a few additional remarks on the consequences that must arise from the admission of this attractive principle.

The nebula, for instance, which has been described at the beginning of this article, as containing several bright nuclei, has probably so many predominant seats of attraction, arising from a superior preponderance of the nebulous matter in those places; but attraction being a principle which never ceases to act, the consequence of its continual exertion upon this nebula will probably be a division of it, from which will arise three or four distinct nebulæ. In the same manner its operation on the diffused nebulosities that have many different bright places, will possibly occasion a breaking up of them into smaller diffusions and detached nebulæ; but before I proceed with conjectures, let us see what observations we have to give countenance to such expectations.

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* See Article 24.

8. *Of double Nebulæ with joined Nebulosity.*

In addition to the instances referred to in the preceding article, of nebulæ that have more than one centre of attraction I give the following list of what may be called double nebulæ.*

The 316th nebula in the second class to which in the catalogue is joined the 317th, consists of " two small faint nebulæ " of an equal size within 1′ of each other. Each has a seeming " nucleus, and their apparent nebulosities run into each " other. Their relative position is in a direction from south " preceding to north following." See fig. 6, *a* and *b*.

Each of the fifteen objects referred to contains two nuclei or centers of attraction, and if the active principle of condensation carries on its operation, a division of their at present united nebulosities must, in the end, be the consequence. I have given two figures for the same double nebulæ. For, although the nebulosities of figure *b*, when seen in the direction of the dotted lines will appear to run together, they may nevertheless be at some small distance from each other; but the same cause which will bring on a separation of it in figure *a* will also make two distinct nebulæ of figure *b*.

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* See I. 56, 176, 178, 193. II. 80, 271, 309, 316, 832. III. 45, 644. IV. 8, 28. *Connoiss.* 27, 51.

With regard to their being double nebulæ, it may be objected that this double appearance may be a deception; and indeed if this were a double star, instead of a double nebula, there might be some room for such a surmise. But on two accounts the case is very different. In the first place, we have not nebulæ without number at all distances to which we might have recourse, in supposing one to be far behind the other, as we have stars behind stars to produce an appearance of their being double. In the next; if what has been said of the confined range of the visibility of the nebulous matter be recollected, especially where it is so faint as in the double nebula which has been described, we cannot harbour an idea that the two objects of which it is composed are very far asunder. Add to this their great resemblance in size, in faintness, in nucleus, and in their nebulous appearance; from all which I believe it must be evident that their nebulosity has originally belonged to one common stock.

9. *Of double Nebulæ that are not more than two Minutes from each other.*

To add to the probability of the separation of nebulæ, we ought to have a considerable number of them already separated. The following twenty-three are completely divided although not more than two minutes from one another.*

A description of II. 714 is "Two pretty bright nebulæ; "they are both round, small, and about 2′ from each other, "in a meridional direction."

Of III. 755 is "Two very faint, very small extended ne-"bulæ within 1½′ from each other."

That all these nebulæ are really double, is founded on the reason already assigned in the last article. Then if we would enter into some kind of examination how they came to be arranged into their binary order, we cannot have recourse to a promiscuous scattering, which by a calculation of chances can never account for such a peculiar distribution of them. If, on the contrary, we look to a division of nebulous matter by the condensing principle, then every parcel of it, which had more than one preponderating seat of attraction in its extent, must in the progress of time have been divided.

No doubt can be suggested on account of the great length of time such a division must have taken up, when we have an eternity of past duration to recur to.

10. *Of double Nebulæ at a greater Distance than 2′ from each other.*

It may well be supposed that more than one attractive center would not be so frequent a case in small distances, as in nebulosities of a more extended compass; accordingly we find that separated nebulæ at more than 2′ from each other are much more numerous. The following 101 double nebulæ referred to will confirm this statement.[*]

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* See I. 116, 190, 197. II. 8, 28, 57, 111, 178, 450, 714. III. 92, 228, 280, 591, 687, 719, 755, 855, 886, 943, 952, 959, 967.

No. 36 and 37 in the first class are "Two small bright "nebulæ, both a little extended."

No. 74 and 75 in the second class are "Two pretty bright "nebulæ; the preceding of them is almost round; the fol-"lowing very much extended in length; they are not far "from the same parallel, and about 8 or 10′ distant from each "other."

No. 127 and 128 in the third class are "Two extremely "faint nebulæ, about 3′ from each other, and nearly in the "same parallel. The second is a very little brighter than the "first, and is of an irregular round figure."

It is remarkable that in the description of all these 101 nebulæ, there are not more than five or six which differ so much in brightness from one another, that we could suppose them to be at any considerable different distance from us; and equal brightness or faintness runs through them all in general; but supposing that any two nebulæ should even differ as much from one another, as the set of the first class which has been described, is different from the faintness of the last described set, yet this would not nearly amount to the difference in the brightness of one part of the nebula in Orion from that of another of the same nebula.

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[*] See I. 28, 36, 90, 145. II. 17, 44, 55, 61, 74, 84, 85, 115, 118, 121, 139, 153, 167, 219, 228, 233, 333, 388, 426, 429, 455, 518, 546, 550, 580, 614, 679, 684, 692, 751, 764, 787, 789, 841, 842, 865, 868. III. 9, 15, 35, 44, 51, 62, 97, 117, 121, 127, 129, 138, 154, 159, 162, 166, 167, 172, 196, 199, 210, 216, 231, 250, 277, 306, 323, 335, 344, 351, 377, 402, 404, 407, 416, 422, 431, 511, 546, 551, 572, 574, 592, 629, 635, 657, 678, 707, 758, 781, 798, 800, 802, 807, 869, 897, 917, 957, 959, 974.

11. *Of treble, quadruple, and sextuple Nebulæ.*

If it was supposed that double nebulæ at some distance from each other would frequently be seen, it will now on the contrary be admitted that an expectation of finding a great number of attracting centers in a nebulosity of no great extent is not so probable; and accordingly observation has shewn that greater combinations of nebulæ than those of the foregoing article are less frequently to be seen. The follow-list however contains 20 treble, 5 quadruple, and 1 sextuple nebulæ of this sort.*

Among the treble nebulæ there is one, namely V. 10, of which the nebulosity is not yet separated. "Three nebulæ "seem to join faintly together, forming a kind of triangle; "the middle of which is less nebulous, or perhaps free from "nebulosity; in the middle of the triangle is a double star of "the 2d or 3d class; more faint nebulosities are following."

Among the quadruple nebulæ we have III. 358. "Four "nebulæ, all within three minutes. The largest is faint and "small; the other three are less and fainter. They form a "small quartile, the largest being the most north of the pre"ceding side."

"The nebulæ which form the sextuple one are all very "faint and very small; they take up a space of more than "10 or 12 minutes."

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* See *treble nebulæ.* I. 17. II. 50, 123, 141, 171, 215, 392, 447. III. 85, 94, 117, 156, 300, 358, 382, 592, 873, 900, 945. V. 10.

Quadruple. II. 482, 568. III. 356, 358, 562.

Sextuple. III. 391.

12. *Of the remarkable Situation of Nebulæ.*

The number of compound nebulæ that have been noticed in the foregoing three articles being so considerable, it will follow, that if they owe their origin to the breaking up of some former extensive nebulosities of the same nature with those which have been shewn to exist at present, we might expect that the number of separate nebulæ should far exceed the former, and that moreover these scattered nebulæ should be found not only in great abundance, but also in proximity or continuity with each other, according to the different extents and situations of the former diffusions of such nebulous matter. Now this is exactly what by observation, we find to be the state of the heavens.

In the following seven assortments we have not less than 424 nebulæ; some of them of unascertained size, figure, or condensation; and the rest with only the first of these three essential features recorded.

The reason for not having a more circumstantial account of such a number of objects, is that they crowded upon me at the time of sweeping in such quick succession, that of sixty-one I could but just secure the place in the heavens, and of the remaining three hundred and sixty-three, I had only time to add the relative size.*

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[*] See *sixty-one nebulæ.* II. 30, 66, 68, 70, 109, 114, 117, 125, 138, 170, 174, 176, 345, 361, 390, 391, 496, 499, 541, 542, 543, 572, 573, 629, 631, 806, 898. III. 20, 26, 31, 33, 39, 41, 42, 89, 103, 189, 193, 205, 332, 353, 363, 364, 365, 390, 413, 432, 481, 482, 483, 484, 485, 669, 670, 705, 796, 819, 930, 934, 936. *Connoiss.* 84.

Ten extremely small nebulæ. III. 98, 108, 194, 195, 230, 238, 297, 526, 545, 639.

One hundred and thirty-six very small nebulæ. II. 22, 64, 67, 72, 91, 93, 287, Neither of the nebulæ in these seven divisions will require a description, as the title of each assortment contains all that has been ascertained about them; but their number and situation, especially when added to those that will be contained in the following articles, completely supports what has been asserted, namely, that the present state of the heavens presents us with several extensive collections of scattered nebulæ, plainly indicating by their very remarkable arrangement, that they

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354, 367, 464, 497, 527, 544, 640, 641, 675, 720, 724, 739, 876. III. 6, 13, 22, 24, 34, 37, 38, 104, 111, 140, 164, 166, 186, 190, 237, 247, 255, 283, 285, 302, 303, 304, 309, 315, 317, 319, 325, 326, 333, 338, 339, 343, 354, 385, 386, 387, 389, 398, 411, 412, 421, 425, 430, 433, 435, 437, 443, 444, 453, 459, 460, 467, 470, 501, 507, 509, 525, 539, 544, 578, 579, 607, 618, 623, 625, 634, 638, 640, 641, 645, 650, 652, 659, 666, 702, 704, 708, 716, 718, 731, 733, 738, 762, 766, 775, 787, 788, 789, 799, 803, 809, 827, 831, 833, 836, 837, 838, 839, 848, 849, 866, 875, 883. 884, 894, 895, 905, 912, 913, 919, 956, 960, 961, 962, 965, 966.

Forty two not very small nebulæ. I. 119. II. 65, 73, 100, 163, 248, 327, 352, 375, 382, 472, 606, 639, 765, 821, 838. III. 17, 30, 249, 281, 321, 327, 366, 375, 504, 548, 615, 628, 647, 660, 667, 698, 712, 715, 734, 751, 773, 774, 840, 850, 941. *Connois.* 89.

One hundred and seven small nebulæ. I. 25, 123. II. 18, 42, 46, 60, 71, 92, 94, 169, 264, 294, 324, 343, 350, 351, 356, 363, 374, 379, 381, 395, 396, 397, 398, 441, 493, 512, 529, 530, 559, 577, 578, 678, 710, 743, 778, 779, 794, 800. III. 25, 48, 57, 59, 60, 69, 74, 192, 206, 235, 243, 308, 328, 329, 334, 337, 350, 380, 420, 446, 458, 462, 464, 475, 478, 502, 516, 517, 529, 550, 588, 611, 651, 661, 664, 668, 721, 722, 723, 729, 761, 763, 769, 779, 780, 794, 797, 814, 826, 833, 841, 843, 861, 880, 881, 894, 915, 924, 925, 926, 927, 928, 939, 950, 951, 954, 969.

Fifty-eight pretty large nebulæ. I. 22, 24, 85, 169, 283. II. 34, 83, 107, 119, 137, 146, 296, 342, 358, 362, 366, 380, 383, 384, 385, 386, 387, 419, 498, 630, 652, 670, 713, 748, 801, 844, 862, 903, 905. III. 14, 18, 40, 70, 75, 76, 102, 213, 261, 279, 318, 340, 367, 372, 374, 415, 454, 473, 503, 543, 599, 662, 790, 970.

Ten large nebulæ. II. 106, 120, 175, 176. III. 28, 361, 440, 480. V. 6. *Connoiss.* 58.

owe their origin to some former common stock of nebulous matter.

To refer astronomers to the heavens for an inspection of these and the following nebulæ, would be to propose a repetition of more than eleven hundred sweeps to them, but those who wish to have some idea of the nebulous arrangements may consult Mr. BODE's excellent Atlas Cœlestis. A succession of places where the nebulæ of my catalogues are uncommonly crowded, will there be seen beginning over the tail of Hydra and proceeding to the southern wing, the body and the northern wing of Virgo, Plate 14. Then to Coma Berenices, Canes venatici, and the preceding arm of Bootes, Plate 7. A different branch goes from Coma Berenices to the hind legs of Ursa major. Another branch passes from the wing of Virgo to the tail and body of Leo, Plate 8.

It will not be necessary to point out many other smaller collections which may be found in several plates of the same Atlas.

On the other hand, a very different aspect of the heavens will be perceived when we examine the following constellations. Beginning from the head of Capricorn, Plate 16, thence proceeding to Antinous, to the tail of Aquila, Plate 9, to Ramus Cerberus, and the body of Hercules, Plate 8, to Quadrans Muralis, Plate 7, and to the head of Draco, Plate 3. We may also examine the constellations of Auriga, Lynx, and Camelopardalus, Plate 5.

In this second review, it will be found that here the absence of nebulæ is as remarkable, as the great multitude of them in the first mentioned series of constellations.

13. *Of very narrow long Nebulæ.*

In order to advance in our knowledge of the condition of the nebulous matter, we may investigate the form of its expansion by the figure of the nebulæ that have been observed. The following five are particular instances of some that were much extended in length, but very little in breadth.*

No. 254 in the 3d class is " A very faint nebula, extended " from north-preceding to south-following. It is about 5′ " long and less than ¼ minute broad." See fig. 7.

The expansion of the nebulous matter in general may be considered as consisting of three dimensions; these may all be either nearly equal, or one of them may be much less than the other two; or the extent of two of them may be very inferior to that of the third. The nebulæ which have now been referred to exclude a nebulosity of three nearly equal dimensions, which can never be seen under less than two of them. When two of the dimensions of the nebulous matter are nearly equal, one of them may indeed be only visible; but then the chance that the other should be exactly parallel to the line of sight, is by no means favourable. The most plausible way of accounting for the apparent figure of these nebulæ is, therefore, to admit that the expansion of the nebulosity consists indeed of a very narrow length, and not much depth. This form when ascribed to nebulous matter, is sufficiently uncommon for us to expect to see many nebulæ of the figure of extended rays.

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* See I. 23, 206. III. 254. IV. 72. V. 20.

14. *Of extended Nebulæ.*

This class of nebulæ, which are chiefly extended in length, but at the same time have a considerable breadth, is very numerous. I have divided the nebulæ it contains, which are 284, into five assortments as follows.*

II. 514 is " A faint nebula extended from south-preceding " to north-following; it is about 2′ long and 1′ broad." See fig. 8.

III. 523 is " A very faint nebula extended from south-pre- " ceding to north-following; it is 3 or 4′ long and nearly 3′ " broad."

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* See *one hundred and sixty-one extended nebulæ of various small sizes.* I. 80, 89, 194, 202, 234. II. 14, 53, 72, 82, 108, 133, 145, 164, 206, 260, 262, 278, 280, 305, 348, 414, 436, 437, 486, 507, 520, 522, 574, 585, 611, 627, 638, 642, 649, 668, 682, 696, 700, 723, 731, 742, 772, 785, 786, 802, 809, 810, 826, 830, 831, 835, 837, 844, 847, 853, 859, 885. III. 4, 23, 56, 58, 65, 66, 73, 79, 82, 100, 110, 132, 183, 218, 225, 236, 241, 242, 244, 248, 258, 265, 305, 313, 314, 316, 342, 347, 348, 355, 369, 370, 406, 410, 419, 427, 429, 441, 442, 445, 450, 479, 487, 490, 494, 496, 499, 510, 514, 515, 520, 521, 528, 554, 557, 567, 569, 570, 586, 598, 599, 601, 612, 613, 619, 646, 649, 653, 677, 681, 682, 713, 714, 727, 730, 732, 752, 767, 771, 778, 783, 792, 804, 806, 808, 811, 812, 813, 816, 832, 845, 846, 874, 885, 892, 904, 914, 920, 929, 932, 942, 948, 949, 973.

Sixty-two extended nebulæ of various large sizes. I. 14, 20, 76, 141, 189, 212, 215, 220, 253. II. 3, 17, 23, 63, 113, 126, 134, 147, 152, 156, 165, 188, 221, 235, 251, 300, 326, 335, 344, 355, 378, 407, 453, 492, 525, 548, 566, 579, 595, 607, 619, 628, 671, 687, 703, 750, 755, 762, 799. III. 253, 282, 290, 346, 414, 492, 498, 508, 610, 689, 740, 766, 776, 921.

Thirty one extended nebulæ from $\frac{3}{4}$ *to 2′ long.* II. 150, 181, 222, 237, 365, 479, 510, 514, 535, 582, 624, 654, 655, 674, 763, 798, 807, 829, 881, 897, 899, 901. III. 203, 368, 506, 556, 620, 648, 692, 906, 907.

Twenty-four extended nebulæ from 2 to 5′ long. I. 94, 174, 201. II. 227, 284, 291, 402, 432, 490, 536, 558, 600, 664, 747, 784, 900. III. 362, 523, 524, 553, 603, 710, 711, 717.

Six extended nebulæ from 5 to 15′ long. I. 134, 153, 285. II. 824. V. 5, 23.

I. 134 is " A considerably bright nebula, 7 or 8 minutes long " and about 3′ broad."

The considerable breadth of these nebulæ, although chiefly extended in length, proves that two of the dimensions of the nebulous matter, namely, the breadth and depth, are probably not very different; for if the depth, which is the dimension we do not see, should be equal to the length, the chance of its being out of sight is not sufficiently probable to happen very frequently. It is therefore to be supposed that the extension in length is really the greatest; for as we actually see it under this form, we are assured that it is at least as long as it appears, whereas one of the other dimensions, if not both, must certainly be less than the length. This kind of expansion admits of the utmost variety of lengthened form and position; and from the great number of nebulæ to which I have referred, the existence of such nebulosities is fairly to be deduced.

15. *Of Nebulæ that are of an irregular Figure.*

Among the various figures that may be seen in nebulæ we have a great many that are of an irregular appearance; I have divided the following ninety-three into two assortments.*

I. 61 is " A very bright small nebula north-following a

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* See *sixty-one irregular nebulæ of various small sizes.* I. 61, 284. II. 185, 242, 259, 274, 281, 306, 339, 415, 445, 586, 597, 601, 605, 647, 744, 761, 834, 886, 893, 907. III. 12, 83, 191, 259, 273, 287, 301, 310, 456, 465, 485, 486, 493, 495, 533, 535, 537, 555, 581, 582, 605, 642, 663, 675, 699, 701, 724, 735, 795, 817, 834, 847, 851, 868, 879, 893, 963, 976, 977.

Thirty-two irregular nebulæ of various large sizes. I. 138, 246, 248, 282. II. 43, 81, 149, 289, 346, 349, 360, 421, 467, 468, 495, 587, 651, 681, 711, 749, 756, 804, 877. III. 137, 257, 274, 463, 683, 695, 765, 911, 938.

" star of the 9th magnitude. It is of an irregular figure." See fig. 9.

II. 289 is " A faint pretty large nebula; it is of an irregular " triangular figure."

By calling the figure of a nebula irregular, it must be understood that I saw no particular dimension of it sufficiently marked to deserve the name of length; for had there been such a distinction, its extension in the longitudinal direction would have been recorded, or, as it frequently happened, for want of time, the nebula would shortly have been called extended. From this consideration it follows, that the nebulous matter which assumes an irregular figure when seen in a telescope, cannot be very different in two of its dimensions; and this leaving the third entirely undetermined, it may be of greater, equal, or less extent than either of the other two. But to be greater or less than the dimensions that were seen it would require the particular situation of the third dimension in either case to be in the direction of the line of sight, which is so far at least improbable, that we may fairly suppose the unseen dimension not to differ much from either of the former two.

16. *Of Nebulæ that are of an irregular round Figure.*

The apparent figure of the nebulæ contained in the foregoing articles has already assisted me in a great measure to assign the expanded form of the nebulous matter of which they consist. The irregular round appearance of the following fifty-five nebulæ however, being of a much more marked description than the former, will lead to more decisive conclusions. I have divided them into three assortments.*

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* See *twenty-eight nebulæ of an irregular round figure of various small sizes.* I.

No. 177 in the third class is "A very faint nebula of an "irregular round figure, about 2 or 3 minutes in diameter." See fig. 10.

The appearance of an irregular round figure necessarily requires that the extent of two dimensions of the nebulous matter should be nearly equal in every direction at right angles to each other. The unseen dimensions may certainly be longer or shorter than the visible irregular diameter; but then it must be absolutely extended centrally in the line of sight, which is a condition that has no probability in its favour; and the greater the number is, of such nebulæ, the less is the probability that the form of the nebulous matter should be irregularly cylindrical, or conical. For, except an irregular cylinder or cone, placed in the particular required situation, no expansion of the nebulous matter but an irregular globular one can be the cause of the irregular round figure of the above-mentioned nebulæ. Then since the irregular globular form has this advantage above the cylindrical and conical figure, that it will answer the required end in any situation whatsoever, it is certainly that which ought to be admitted as the cause of the observed appearance.

This method of reasoning upon the form of the nebulous matter from the observed figure of nebulæ, will lead us a step farther than it might have been supposed. For granting it to

231. II. 97, 191, 243, 254, 273, 336, 560, 758, 895, 896. III. 208, 224, 311, 474, 565, 600, 614, 621, 673, 674, 688, 728, 784, 813, 835, 931, 955.

Twenty-one nebulæ of an irregular round figure of various large sizes. I. 69, 108, 161. II. 197, 240, 494, 513, 537, 538, 552, 685, 727, 872, 890. III. 426, 447, 558, 862, 876. V. 7. *Connoiss.* 70.

Six nebulæ of an irregular round figure of a mean diameter from 1 *to* 5′. III. 131, 177, 223, 261, 542, 617.

be highly probable, that the appearance of irregular round nebulæ are owing to so many irregular globular expansions of nebulous matter, it will be necessary to direct our attention to the cause which has formed this matter into such masses. To ascribe an highly improbable event to chance is not philosophical; especially as a forming cause offers itself to our view, when we direct an eye to the globular figure of the planets and satellites of the solar system.

17. *Of round Nebulæ.*

From what has been said, it appears that the figure of nebulæ is a subject of more interest than mere curiosity. The following fifty-seven were observed to be round, and I give them here in four assortments.*

As the title of each sort gives all that is necessary for the present purpose relating to the various sizes of round nebulæ, a description of one of the last will be sufficient. The observation of I. 269 says, that it is "A considerably bright round nebula of about one minute in diameter." See fig. 11.

The arguments which I have given in the foregoing article, where only nebulæ of an irregular round figure were considered, need not be repeated when a regular circular form is presented to our view; for the additional number of nebulæ, and the regularity of their figure are both greatly in favour of a conclusion, that the mass of the nebulous matter which occasions their appearance must be of a globular form.

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* See *three round nebulæ.* III. 381, 511, 754.

Forty-one round nebulæ of various small sizes. I. 275. II. 54, 218, 223, 225, 329, 659, 760, 803. III. 11, 50, 78, 94, 95, 96, 149, 150, 180, 181, 209, 221, 222, 295, 371, 451, 477, 505, 622, 631, 671, 684, 726, 760, 800, 801, 810, 842, 888, 909, 946, 971.

Ten round nebulæ of various large sizes. I. 7, 124, 252. II. 19, 481, 889. III. 54, 77, 112, 452.

Three round nebulæ from 1 to 6' in diameter. I. 269. II. 593. V. 16.

In the last article I have only directed our attention to the cause of this very particular construction, but from the observations of the nebulæ above referred to, we may now more confidently assign the attraction of gravitation as the principle which has drawn the nebulous matter towards a center, and collected it into a spherical compass.

I have already shewn that the same principle appears to be the cause of the condensation of the nebulous matter in the bright places of nebulæ that shine with unequal degrees of light in the different parts of their extent,[*] and a concurrence of arguments established upon very different foundations cannot fail to give additional weight to the reasonings by which they are supoprted.

18. *Of Nebulæ that are remarkable for some particularity in Figure or Brightness.*

Among the nebulæ, which I have described as of an irregular figure, the following might have been inserted; but the real form of the nebulous matter of which they consist is probably as irregular as the figure or brightness of the nebulæ themselves. I have arranged thirty-five of them into three assortments.[†]

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[*] See Article 7.

[†] See *two nebulæ of remarkable figure.* I. 286. V. 19.

Ten unequally bright nebulæ. I. 254. II. 200, 210, 422, 557, 591, 646. III. 142, 145, 534.

Twenty-three nebulæ that are brightest on one side. I. 113, 162. II. 26, 27, 136,

V. 19 is "A considerably bright nebula about 15′ long and "3′ broad; its length is divided in the middle by a black division at least three or four minutes long." See fig. 12.

The nebulous matter of this nebula is probably a ring in a very oblique position with respect to the line of sight.

II. 646 is "A pretty bright, large nebula, of an irregular "figure; it is unequally bright."

The inequally of its brightness in different parts may arise from unequal condensation or from greater depth of nebulous matter.

II. 313 is "A pretty bright nebula, a little extended in the "parallel. The greatest brightness is towards the following "side, which is also the broadest; the preceding part being "more like a ray proceeding from it."

The irregular figure of these latter kind of nebulæ may be admitted to arise from the as yet imperfect concentration of a nebulous mass, in which the preponderating matter of it is not in the center.

19. *Of Nebulæ that are gradually a little brighter in the middle.*

The investigation of the form of the nebulous matter in the 13, 14, 15, and 16th articles has been founded only upon the observed figure of nebulæ; and in the 17th article the globular form of this matter deduced from the round appearance of nebulæ, has been ascribed to the action of the gravitating principle. I am now entering upon an examination of nebulæ of which, besides their figure, I have also recorded the different degrees of light, and the situation of the greatest brightness

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155, 313, 332, 364, 369, 370, 442, 506, 531, 555, 589, 623. III. 120, 153, 286, 676, 700. V. 22.

with respect to their figure. These observations will establish the former conclusions by an additional number of objects, and by the decisive argument of their brightness, which points out a seat of attraction.

In the following four assortments are one hundred and fifty nebulæ, which all agree in being a little brighter in the middle. This increase of brightness must be understood to be always very gradual from the outside towards the middle of the nebula, whatever be its figure; and although this circumstance, for want of time, has often been left unnoticed in the observation, I am very sure that had the gradation of brightness been otherwise, it would certainly not have been overlooked.*

III. 853 is "A very faint small nebula; it is very gradually "a little brighter in the middle."

III. 488 is "A very faint extended nebula, near 3′ long,

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* See *Thirty-two nebulæ, the particular figure of which has not been ascertained, gradually a little brighter in the middle.* II. 201, 401, 424, 444, 457, 528, 532, 616, 617, 648, 673, 677, 736, 904. III. 90, 106, 148, 331, 436, 472, 489, 519, 596, 633, 654, 655, 656, 686, 853, 860, 896, 978.

Twenty-four extended nebulæ, gradually a little brighter in the middle. II. 184, 192, 252, 285, 412, 478, 480, 565, 621, 688, 906. III. 141, 233, 449, 461, 468, 488, 532, 577, 736, 890. V. 8, 40, 50.

Twenty nebulæ of an irregular figure, gradually a little brighter in the middle. II. 213, 357, 403, 471, 487, 491, 524, 533, 594, 717, 729. III. 272, 428, 434, 626, 690, 857, 903, 947. V. 29.

Seventy-four round or nearly round nebulæ, gradually a little brighter in the middle. II. 7, 40, 102, 129, 131, 162, 190, 249, 258, 267, 276, 286, 290, 308, 320, 338, 428, 459, 474, 476, 477, 509, 516, 526, 602, 637, 699, 725, 737, 770, 780, 797, 811, 812. III. 62, 63, 94, 105, 121, 122, 123, 133, 162, 163, 252, 292, 296, 298, 330, 388, 409, 448, 466, 497, 522, 597, 608, 665, 680, 746, 750, 753, 818, 822, 823, 824, 858, 867, 889, 891, 908, 917, 918, 923.

" and above 2′ broad; it is gradually a little brighter in the " middle. Fig. 13.

II. 549 is " A very large and pretty bright nebula of an " irregular figure; it is a little brighter in the middle." Fig. 14.

II. 812 is " A faint, small, round nebula; it is very gradu" ally a little brighter in the middle, and the increase of " brightness begins at a distance from the center." Fig. 15.

It is hardly necessary to say that the united testimony of so many objects can leave no doubt about the central seat of attraction, which in every instance of figure is pointed out to be in the middle.

The only remark I have to make, relates to the exertion of the condensing power, which in the case of these nebulæ appears to have produced but a very moderate effect. This may be ascribed either to the unshapen mass of nebulous matter which would require much time before it could come to some central arrangement of form either in length, or in length and breadth, or lastly in all its three dimensions. It may also be ascribed to the small quantity of the preponderating central attractive matter; or even to the shortness of its time of acting: for in this case millions of years, perhaps are but moments.

20. *Of Nebulæ which are gradually brighter in the middle.*

By the general description of a nebula, when it is said to be gradually brighter in the middle, we are to understand that its light was observed to be obviously brighter about the center than in other parts. Had the nebulæ of this class been only a little brighter, or had they been much brighter in the middle, such additional expressions would certainly have been used; except where time would not allow to be more particular. I have sorted two hundred and twenty-three of these nebulæ like the foregoing, according to their figure, into four classes.*

II. 409 is " A pretty bright and pretty large nebula; it is " very gradually brighter in the middle."

I. 55 is " A considerably bright, extended nebula about 4′ " long and 2′ broad, in a meridional direction; it is gradually " brighter in the middle." Fig. 16.

I. 266 is " A considerably bright, and pretty large nebula, " of an irregular figure; it is gradually brighter in the mid- " dle." Fig. 17.

I. 98 is " A considerably bright, and pretty large round " nebula; it is brighter in the middle, the brightness diminish-

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* See *Thirty-nine nebulæ of an unascertained figure, gradually brighter in the middle.* I. 19, 49, 264. II. 24, 49, 87, 88, 89, 90, 319, 337, 347, 368, 373, 409, 440, 515, 534, 590, 610, 634, 636, 672, 783, 830, 840, 856, 857, 858, 860, 861, 863. III. 275, 584, 587, 602, 872, 892, 935.

Fifty extended nebulæ gradually brighter in the middle. I. 1, 55, 62, 131, 199, 241, 259, 263, 279. II. 1, 10, 52, 77, 95, 132, 135, 157, 203, 205, 211, 253, 266, 302, 325, 405, 417, 508, 539, 545, 583, 592, 613, 625, 643, 656, 667, 697, 709, 730, 773, 880, 882. III. 246, 267, 589, 594, 864, 902. V. 4. 39.

Twenty-nine nebulæ of an irregular figure, gradually brighter in the middle. I. 95, 196, 227, 266. II. 36, 56, 96, 130, 226, 265, 295, 314, 353, 423, 433, 434, 475, 488, 553, 596, 657, 663, 690, 793, 819, 825, 887. III. 397, 500.

One hundred and five round, or nearly round nebulæ, gradually brighter in the middle. I. 5, 12, 54, 70, 98, 106, 120, 148, 168, 186, 211, 222, 229, 243, 245, 274. II. 50, 51, 128, 151, 158, 160, 161, 196, 208, 224, 247, 255, 256, 263, 275, 293, 307, 312, 330, 331, 333, 359, 376, 399, 408, 411, 435, 458, 461, 465, 511, 517, 523, 562, 567, 580, 588, 594, 614, 615, 622, 632, 633, 635, 662, 712, 719, 741, 769, 777, 792, 817, 818, 845, 851, 852, 865, 866, 873, 879, 883, 884, 888, 902. III. 2, 88, 107, 138, 139, 220, 491, 527, 541, 609, 694, 739, 749, 825, 829, 865, 870, 871, 882, 899, 900, 933, 937, 940, 972.

" ing very gradually from the center towards the circum-" ference." Fig. 18.

From the account of these nebulæ, we find again that all what has been said concerning the seat of the forming and condensing power of the nebulous matter, is abundantly confirmed by observation.

I have only to remark that, the exertion of the gravitating principle in these nebulæ, is in a more advanced state than with those of the last article; and that the same conceptions which have already been suggested, namely, the original form of the nebulous matter; its quantity in the seat of the attracting principle; and the length of the time of its action, when properly considered, will sufficiently account for the present state of these nebulæ.

21. *Of Nebulæ that are gradually much brighter in the middle.*

The nebulous matter which appears under the various forms of the following four assortments, containing two hundred and two nebulæ, assumes now a more condensed aspect, than that under which it was seen in either of the two foregoing collections; and thus by its gradually greater compression, gives us a still more decisive indication of the central seat of attraction.[*]

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[*] See *Twenty-five nebulæ of unascertained figure, gradually much brighter in the middle.* I. 73. 121, 127, 140, 155, 181, 287. II. 35, 177, 187, 299, 439, 452, 540, 653, 658, 669, 686, 694, 795, 828, 855, 871. III. 863. *Connoiss.* 99.

Fifty-four extended nebulæ, gradually much brighter in the middle. I. 29, 31, 33, 35, 38, 53, 58, 64, 72, 82, 86, 93, 97, 101, 104, 125, 154, 157, 164, 184, 209, 233, 239, 274, 271, 277. II. 12, 13, 31, 37, 182, 212, 231, 282, 318, 416, 431, 463, 504, 604, 612, 626, 691, 701, 702, 704, 725, 753, 775, 875. III. 179. 198. V. 47. *Connoiss.* 49.

II. 828 is "A pretty bright small nebula, very gradually "much brighter in the middle."

I. 101 is "A considerably bright pretty large nebula, ex-"tended in the meridional direction, about 4′ or 5′ long; much "brighter in the middle." In the 40 feet telescope I saw the very gradual increase of brightness towards the middle of its length; a longer extent of the nebula was also visible. Fig. 19.

I. 219 is "A very bright considerably large nebula of an "irregular figure, very gradually much brighter in the mid-"dle." Fig. 20.

I. 63 is "A bright round nebula of about one minute in "diameter; it is much brighter in the middle, and very faint "towards the border." Fig. 21.

The greater difference between the comparative brightness of the center, and the outward parts of these nebulæ, may certainly be ascribed to the same causes that have been considered in the two foregoing articles; but in the present case, and taking into the account that this is already a third step of condensation from a little brighter to brighter; then, to much

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Nineteen nebulæ of an irregular figure, gradually much brighter in the middle. I. 10, 26, 59, 66, 109, 110, 114, 115, 219, 235, 237, 276. II. 2, 20, 438, 503, 734, 827. III. 299.

One hundred and four round or nearly round nebulæ, gradually much brighter in the middle. I. 8, 16, 21, 30, 42, 63, 65, 67, 68, 74, 79, 83, 87, 88, 100, 102, 105, 111, 112, 118, 129, 135, 136, 142, 144, 147, 150, 158, 159, 166, 171, 175, 182, 185, 216, 218, 221, 232, 238, 244, 257, 260 265, 273, 278. II. 5, 11, 38, 69, 98, 148, 230, 236, 245, 250, 257, 269, 270, 277, 288, 292, 301, 303, 309. 311, 328, 418, 420, 446, 462, 466, 556, 561, 564, 575, 598, 632, 644. 645, 600, 656. 695, 707, 728, 738, 757, 767, 774, 782, 816, 823, 839, 854, 874. III. 250, 284, 512, 531, 624, 744, 859, 878. *Connoiss.* 59, 96.

brighter, there appears to be some foundation for supposing rather that this greater effect is produced by a longer time of the action of the attractive principle, than that it should arise merely from an original more favourable expansion of the nebulous matter.

22. *Of Nebulæ that have a Cometic appearance.*

Among the numerous nebulæ I have seen, there are many that have the appearance of telescopic comets. The following are of that sort.*

I. 4 is "A pretty large cometic nebula of considerable "brightness; it is much brighter in the middle, and the very "faint chevelure is pretty extensive." Fig. 22.

By the appellation of cometic, it was my intention to express a gradual and strong increase of brightness towards the center of a nebulous object of a round figure; having also a faint chevelure or coma of some extent, beyond the faintest part of the light, gradually decreasing from the center.

It seems that this species of nebulæ contains a somewhat greater degree of condensation than that of the round nebulæ of the last article, and might perhaps not very improperly have been included in their number. Their great resemblance to telescopic comets, however, is very apt to suggest the idea, that possibly such small telescopic comets as often visit our neighbourhood may be composed of nebulous matter, or may in fact be such highly condensed nebulæ.

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* See *Seventeen cometic nebulæ.* I. 3, 4, 34, 217. II. 6, 15, 33, 59, 104, 153, 154, 241, 315, 404. III. 5, 21. *Connoiss.* 95.

23. *Of Nebulæ that are suddenly much brighter in the middle.*

From the third degree of visible condensation, I have in the 21st article intimated, that the length of the time of the action of the attracting principle, would explain the observed gradual accumulation of the nebulous matter. In the following eighteen nebulæ we may see a still more advanced compression of it, amounting almost to the appearance of a nucleus.*

II. 814 is "A small faint nebula, very suddenly much "brighter in the middle."

I. 39 is "A very bright nebula, extended from south-preceding to north-following, about 4′ or 5′ long, and 3′ broad; it is much brighter in the middle, but the brightness breaks off abruptly, so as almost to resemble a nucleus." Fig. 23.

I. 256 is "A very bright pretty large nebula of an irregular "figure; it is suddenly much brighter in the middle." Fig. 24.

I. 99 is "A very bright, small, round nebulæ; it is very "suddenly much brighter in the middle." Fig. 25.

From the appearance of these nebulæ, we see plainly that a progressive concentration of the nebulous matter has an existence; it is also remarkable that the condensation in long nebulæ inclines towards the shape of a nucleus, as well as in round ones, which can be ascribed only to the continued action of the attracting principle, tending to draw the nebulous extended expansion into a globular form.

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* See *One nebula of unascertained figure, suddenly much brighter in the middle.* II. 814.

Seven extended nebulæ, suddenly much brighter in the middle. I. 39, 91, 96, 200, II. 183, 505. *Connoiss.* 66.

Two nebulæ of an irregular figure, suddenly much brighter in the middle. I. 256. II. 521.

Eight round or nearly round nebulæ, suddenly much brighter in the middle. I. 99, 138. II. 410, 413, 698. III. 251, 685. *Connoiss.* 54.

A nucleus, to which these nebulæ seem to approach, is an indication of consolidation; and should we have reason to conclude that a solid body can be formed of condensed nebulous matter, the nature of which has hitherto been chiefly deduced from its shining quality, we may possibly be able to view it with respect to some other of its properties.

24. *Of round Nebulæ increasing gradually in brightness up to a Nucleus in the middle.*

It has already been proved, from the figure and central brightness of round nebulæ, that the nebulous matter of which they consist must be admitted to be of a globular form; but the following thirteen nebulæ lead me to a remark which not only applies to them, but to all the round nebulæ of the last five articles, which added to these amount to three hundred and twenty-one. They are not only round, but the gradual condensation from the circumference to the very center being of equal density of light at equal central distances, every ring or circle drawn round the center, bears witness to the existence of a central attraction. For whatever may be the intensity or ratio of the concentration at any given central distance, it follows, from the equality of brightness at the assigned distance, that no figure but a globular one can with any kind of probability explain the appearance; and that the concentration, as well as the figure, is produced by a general gravitation of the nebulous matter.*

* See I. 2, 6, 132, 151, 173, 236, 272. II. 25, 189, 716, 864. III. 518. IV. 6.

I. 151 is " A considerably bright and considerably large, " round cometic nebula; it is very gradually much brighter " in the middle, with a nucleus in the center." Fig. 26.

From the description of these nebulæ, we find that an actual nucleus has been formed in the attracting center; and that consequently a certain degree of consolidation of the nebulous matter is highly probable; for, although the quality of shining only points out the existence of something that is luminous, yet from analogy we have reason to conclude that certain material substances must be present to produce the light we perceive; and that they must be opaque, may be inferred from every thing we know about shining substances.

25. *Of Nebulæ that have a Nucleus.*

It may be expected that some considerable change will take place in the appearance of a nebula after it has come to a certain degree of continued gradual condensation. We are as yet so little acquainted with the nature and distribution of this matter, that an application of mathematical calculations, founded on the attraction of gravitation, for want of data, cannot be applied in order to suggest to us what appearance might next be expected; I shall therefore proceed in a regular manner to give the observations, which shew what these appearances are, without entering into any theoretical discussions.

In the following two assortments we have forty nebulæ.*

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* See *Twenty-seven extended nebulæ, with a nucleus.* I. 43, 77, 126, 156, 170, 180, 208, 224, 240, 250, 255, 270, 280, 281. II. 238, 460, 759, 768, 795, 846, 849, 891. V. 18, 24, 48. *Connoiss.* 63, 101.

Thirteen round or nearly round nebulæ, with a nucleus. I. 107, 133, 139. 152, 167, 203, 225. II. 99, 501, 746, 754. III. 178. *Connoiss.* 90.

Number 63 of the *Connoissance des Temps* is "A very "bright nebula, extending from north-preceding to south-"following 9 or 10′ long, and near 4′ broad; it has a very "brilliant nucleus." Fig. 27.

I. 107 is "A very bright round nebula, about 1½ minutes "in diameter; it has a bright nucleus in the middle." Fig. 28.

The nuclei of these nebulæ, after what has been proved, of the existence of a condensing power, I need not hesitate to ascribe to the longer continuance of its action, which appears to bring on a consolidation; and that this may be the consequence we may conclude, not only from the power of condensing, which argues a sufficient quantity of matter, but also from the quality of shining; for this proves that the substance which throws out the nebulous light is endowed with some other of the general qualities of matter besides that of gravitation.

A second remark I have to make is, that the opaque nature of the nebulous matter which was before inferred from analogy, is here supported by observation; for these consolidated nuclei have a considerable resemblance to the disks of planets; and if this matter consisted only of a luminous substance, the increase of light would probably far exceed their observed lustre: this being the case, the power of arresting light in its passage is an additional quality, very different from those which have already been mentioned, and seems to be analogous to properties which we know to belong to hard and solid bodies.

26. *Of extended Nebulæ that shew the Progress of Condensation.*

When the nebulous matter is much extended in length, it appears from the following nebulæ, that with those which have a nucleus completely formed, the nebulosity on each side of it is comparatively reduced to a fainter state than it is in nebulæ of which the nucleus is apparently still in an incipient state. These faint opposite appendages to the nucleus I have in my observations called branches.

In some nebulæ there is also an additional small faint nebulosity of a circular form about the nucleus, and this I have called the chevelure. The following two assortments contain twenty-eight nebulæ of this kind.*

Number 65 of the *Connoissance* is " A very brilliant nebula " extended in the meridian, about 12′ long. It has a bright " nucleus, the light of which suddenly diminishes on its " border, and two opposite very faint branches." Fig. 29.

I. 205 is " A very brilliant nebula, 5′ or 6′ long and 3 or 4′ " broad; it has a small bright nucleus with a faint chevelure " about it, and two opposite very extensive branches. Fig 30.

The construction of these nebulæ is certainly complicated and mysterious, and in our present state of kowledge it would be presumptuous to attempt an explanation of it; we can only form a few distant surmises, which however may lead to the following queries. May not the faintness of the branches arise from a gradual diminution, of the length and density of the nebulous matter contained in them, occasioned by its gravitation towards the nucleus into which it probably subsides? Are not these faint nebulous branches joining to a nucleus, upon an immense scale, somewhat like what the zodiacal light is to our sun in miniature? Does not the chevelure denote that perhaps some of the nebulous matter still remaining in the branches, before it subsides into the nucleus, begins to take a spherical form, and thus assumes the semblance of a faint chevelure surrounding it in a concentric arrangement? And, if we may venture to extend these queries a little farther—will not the matter of these branches in their gradual fall towards the nucleus, when discharging their substance into the chevelure, produce a kind of vortex or rotatory motion? Must not such an effect take place, unless we suppose, contrary to observation, that one branch is exactly like the other; that both are exactly in a line passing through the center of the nucleus, by way of causing exactly an equal stream of it from each branch to enter the chevelure at opposite sides; and, this not being probable, do we not see some natural cause which may give a rotatory motion to a celestial body in its very formation?

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* See *Twenty-three extended nebulæ with a nucleus and two opposite faint branches.* I. 9, 13, 15, 27, 32, 75, 130, 160, 163, 187, 188, 195, 223, 228, 230. II. 101, 650, 733. IV. 61. V. 43. *Connoiss.* 65, 83, 98.

Five with a nucleus, chevelure and branches. I. 174, 205, 210. V. 45. *Connoiss* 94.

27. *Of round Nebulæ that shew the Progression of Condensation.*

When round nebulæ have a nucleus, it is an indication that they have already undergone a high degree of condensation. From their figure we are assured that the form of the nebulosity of which they are composed is now spherical, whatever may have been its original shape; and being surrounded by a chevelure, we may look upon its different evanescent degrees of faintness as a sign whereby to judge of the gradual progress of the consolidation of the nucleus. The following seventeen nebulæ are given in two assortments.*

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* See *fifteen round or nearly round nebulæ, with a nucleus and faint chevelure.* I. 40, 137, 226, 242, 251, 262. II. 321. III. 291, 373. IV. 23, 54, 56, 59, 76. *Connoiss.* 32.

Two nebulæ with a nucleus and chevelure resembling nebulous stars. II. 32. III. 99.

IV. 23 is "A considerably bright nebula with a very bright nucleus, and a chevelure about 3 or 4′ in diameter." Fig. 31.

III. 99 is "A small nebula with a pretty bright nucleus and "very faint chevelure; it is almost like a nebulous star." Fig. 32.

The chevelure of these nebulæ consists probably of the rarest nebulous matter, which not having as yet been consolidated with the rest, remains expanded about the nucleus in the shape of a very extended atmosphere; or it may be of an elastic nature, and be kept from uniting with the nucleus, as their elasticity causes the atmospheres of the planets to be expanded about them. In this case we have another property of the nebulous substance to add to the former qualities of its matter.

With those nebulæ where this chevelure is uncommonly faint, and the nucleus very bright, the consolidation appears to have reached a still higher degree, and their resemblance to nebulous stars may lead to very interesting consequences.

28. *Of round Nebulæ that are of an almost uniform Light.*

The argument that the nebulous matter is in some degree opaque which is given in the 25th article, will receive considerable support from the appearance of the following nebulæ; for they are not only round, that is to say the nebulous matter of which they are composed is collected into a globular compass, but they are also of a light which is nearly of an uniform intensity except just on the borders. I give these nebulæ in two assortments.*

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* See *Four from 2′ to 4′ in diameter.* IV. 50, 62, 67. *Connoiss.* 97.
Twelve nebulæ from ¼ of a minute to 2′ in diameter. I. 267. II. 186, 209, 705, 836, 870. III. 152, 877. IV. 13, 14, 16, 39.

Number 97 of the *Connoissance* is " A very bright, round " nebula of about 3′ in diameter; it is nearly of equal light " throughout, with an ill defined margin of no great extent."

IV. 13 is " A pretty faint nebula of about 1′ diameter; it is " perfectly round, and of an equal light throughout; and the " edges of it are pretty well defined." Fig. 33.

Admitting that these sixteen nebulæ are globular collections of nebulous matter, they could not appear equally bright, if the nebulosity of which they are composed consisted only of a luminous substance perfectly penetrable to light; at least this could not happen unless a certain artificial condensation of it were introduced, which can have no pretension to probability in its favour. Is it not rather to be supposed, that a certain high degree of condensation has already brought on a sufficient consolidation to prevent the penetration of light, which by this means is reduced to a superficial planetary appearance?

29. *Of Nebulæ that draw progressively towards a Period of final Condensation.*

In the course of the gradual condensation of the nebulous matter, it may be expected that a time must come when it can no longer be compressed, and the only cause which we may suppose to put an end to the compression is, when the consolidated matter assumes hardness. It remains therefore to be examined, how far my observations will go to ascertain the intensity of its consolidation.

The following two assortments contain seven nebulæ, from whose appearance a considerable degree of solidity may be inferred.*

IV. 55 is "A pretty bright round nebula, almost of an even "light throughout approaching to a planetary appearance, "but ill defined, and a little fainter on the edges; it is about "$\frac{3}{4}$ or 1 minute in diameter." Fig. 34.

IV. 37 is "A very bright planetary disk of about 35″ in "diameter, but ill defined on the edges; the center of it is "rather more luminous than the rest, and with long attention "a very bright well defined round center becomes visible." Fig. 35.

In these nebulæ we have three different indications of the compression of the nebulous matter of which they are composed: their figure, their light, and the small compass into which it is reduced. The round figure is a proof that the nebulous mass is collected into a globular form, which cannot have been effected without a certain degree of condensation.

Their planetary appearance shews that we only see a superficial lustre such as opaque bodies exibit, and which could not happen if the nebulous matter had no other quality than that of shining, or had so little solidity as to be perfectly transparent. That there is a certain maximum of brightness occasioned by condensation, is to be inferred from the different degrees of light of round nebulæ that are in a much less advanced state of compression; for these are gradually more bright towards the center; which proves that brightness keeps up with condensation till the increase of it brings on a consolidation which will no longer permit the internal penetration of light, and thus a planetary appearance must in the end be the consequence; for planets are solid opaque bodies, shining only by superficial light, whether it be innate or reflected.

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* See *Four nebulæ of a planetary appearance.* IV. 55, 60, 68, 78.
Three planetary disks with a bright central point. II. 268, IV. 37, 73.

From the size of the nebulæ as we see them at present, we cannot form an idea of the original bulk of the nebulous matter they contain; but let us admit, for the sake of computation, that the nebulosity of the above described nebula IV. 55, when it was in a state of diffusion, took up a space of 10′ in every cubical direction of its expansion; then, as we now see it collected into a globular compass of less than one minute, it must of course be more than nineteen hundred times denser than it was in its original state. This proportion of density is more than double that of water to air.

With regard to planetary disks, which have bright central points, we may surmise that their original diffused nebulosity was more unequally scattered, and that they passed through the different stages of extended nebulæ, gradually acquiring a nucleus, chevelure, and branches. For in nebulæ of this construction, the consolidation of a nucleus is already much advanced at the time when a considerable quantity of nebulous matter, on account of its greater central distance, remains still unformed in the branches; and if the condensation of the nucleus should keep the lead, it will come to a state of great solidity and maximum of brightness by the time that the rest of the nebulosity is drawn into a planetary appearance.

30. *Of Planetary Nebulæ.*

The objects of which I shall give an account in this article have so near a resemblance to planets, that the name of planetary nebulæ very justly expresses their appearance; for notwithstanding their planetary aspect, some small remaining haziness, by which they still are more or less surrounded, evinces their nebulous origin. In my catalogues the places of the following ten have been given.*

IV. 18 is " A beautiful bright round nebula, having a pretty " well defined planetary disk of about 10 or 12″ in diameter. " It is a little elliptical, and has a very small star following, " which gives us the idea of a small satellite accompanying " its planet. It is visible in a common finder as a small star." Fig. 36.

IV. 27 is " A beautiful very brilliant globe of light, hazy " on the edges, but the haziness going off suddenly. I sup- " pose it to be from 30 to 40″ in diameter, and perhaps a very " little elliptical. The light of it seems to be all over of the " uniform lustre of a star of the 9th magnitude. The hazi- " ness on the edges does not exceed the 20th part of the " diameter."

IV. 51 is " A small beautiful planetary nebula, but consi- " derably hazy upon the edges: it is of a uniform light, and " considerably bright, perfectly round, and about 10 or 15″ in " diameter."

IV. 53 is " A pretty bright planetary nebula of nearly 1′ in " diameter; it is round, or a little elliptical; its light is uni- " form, and pretty well defined on the borders."

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* See *Planetary nebulæ* IV. 1, 11, 18, 26, 27, 34, 51, 53, 64, 70.

IV. 64 is "A beautiful planetary nebula of a considerable "degree of brightness, but not very well defined, about 12 "or 15″ in diameter."

The remarks which have been made on the nebulæ of the foregoing article, will here apply with additional propriety: for the light of these planetary nebulæ must be considerably more condensed than that of the foregoing sets. The diameter of four of them does not exceed 15,″ so that if we again suppose the original diffused nebulosity from which they sprang of 10′ in cubical dimensions, we shall have a condensation, which has reduced the nebulous matter to less than the one-hundred and twenty-two thousandth part of its former bulk.

One of them, number 34 in the 4th class, appeared even in the 20 feet telescope, with the sweeping power, like a star with a large diameter, and it was only when magnified 240 times that it resembled a small planetary nebula; nor can any of these nebulæ be distinguished from the neighbouring small stars in a good common telescope, night glass, or finder.

When we reflect upon these circumstances, we may conceive that, perhaps in progress of time these nebulæ which are already in such a state of compression, may be still farther condensed so as actually to become stars.

It may be thought that solid bodies, such as we suppose the stars to be from the analogy of their light with that of our sun when seen at the distance of the stars, can hardly be formed from a condensation of nebulous matter; but if the immensity of it required to fill a cubical space, which will measure ten minutes when seen at the distance of a star of the 8th or 9th magnitude, is well considered, and properly compared with the very small angle our sun would subtend at the same distance, no degree of rarity of the nebulous matter, to which we may have recourse, can be any objection to the solidity required for the construction of a body of equal magnitude with our sun.*

A circumstance which allies these very compressed nebulæ to the character of many of our well known celestial bodies, such as some of the planets and their satellites, the sun and all periodical stars, is that very probably most, if not all of them, turn on their axes. Seven of the ten I have mentioned are not perfectly round, but a very little elliptical. Ought we not to ascribe this figure to the same cause which has flattened the polar diameter of the planets, namely, a rotatory motion?

At the end of the 26th article I have already pointed out one configuration of the nebulous matter, of which the final condensation seems to be properly disposed for bringing on a rotatory motion of the nucleus; but, if we consider this matter in a general light, it appears that every figure which is not already globular must have eccentric nebulous matter, which in its endeavour to come to the center, will either dislodge some of the nebulosity which is already deposited, or slide upon it sideways, and in both cases produce a circular motion; so that in fact we can hardly suppose a possibility of the production of a globular form without a consequent revolution of the nebulous matter, which in the end may settle in a regular rotation about some fixed axis. Many of the extended, and irregular nebulæ are considerably elliptical, and the irregular round ones shew a general approach to the oval form; now these figures are all favourable to a surmise, that a rotatory motion may often take place even before the nucleus of a nebula can have arrived to a state of consolidation. An objection, that this remarkable form of planetary nebulæ may be owing to chance, will hardly deserve to be mentioned, because the improbability of such a supposition must exclude it from all claim to refutation.

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* A cubical space, the side of which at the distance of a star of the 8th magnitude is seen under an angle of 10′, exceeds the bulk of the sun (2208600000000000000.0) two trillion and 208 thousand billion times.

31. *Of the Distance of the Nebula in the Constellation of Orion.*

In my 3d article I concluded, from the appearance of the great nebula in Orion, that the range of the visibility of the diffused nebulous matter cannot be great, because we may there see in one and the same object, both the brightest and faintest appearance of nebulosities that can be seen any where. It will therefore be a case of some interest, if we can form any conception of the place among the fixed stars to which we ought to refer the situation of this nebula; and this I believe my observation of it will enable us to determine pretty nearly.

In the year 1774, the 4th of March, I observed the nebulous star, which is the 43d of the *Connoissance des Temps*, and is not many minutes north of the great nebula; but at the same time I also took notice of two similar, but much smaller nebulous stars; one on each side of the large one, and at nearly equal distances from it. Fig. 37 is a copy of a drawing which was made at the time of observation.

In 1783, I examined the nebulous star, and found it to be faintly surrounded with a circular glory of whitish nebulosity, faintly joining to the great nebula.

About the latter end of the same year I remarked that it was not equally surrounded, but most nebulous towards the south.

In 1784 I began to entertain an opinion that the star was not connected with the nebulosity of the great nebula of Orion, but was one of those which are scattered over that part of the heavens.

In 1801, 1806, and 1810 this opinion was fully confirmed, by the gradual change which happened in the great nebula, to which the nebulosity surrounding this star belongs. For the intensity of the light about the nebulous star had by this time been considerably reduced, by the attenuation or dissipation of the nebulous matter; and it seemed now to be pretty evident that the star is far behind the nebulous matter, and that consequently its light in passing through it is scattered and deflected, so as to produce the appearance of a nebulous star. A similar phenomenon may be seen whenever a planet or a star of the 1st or 2nd magnitude happens to be involved in haziness; for a diffused circular light will then be seen, to which, but in a much inferior degree, that which surrounds this nebulous star bears a great resemblance.

When I reviewed this interesting object in December 1810, I directed my attention particularly to the two small nebulous stars, by the sides of the large one, and found that they were perfectly free from every nebulous appearance; which confirmed not only my former surmise of the great attenuation of the nebulosity, but also proved that their former nebulous appearance had been entirely the effect of the passage of their feeble light through the nebulous matter spread out before them.

The 19th of January 1811, I had another critical examination of the same object in a very clear view through the 40-feet telescope; but notwithstanding the superior light of this instrument, I could not perceive any remains of nebulosity about the two small stars, which were perfectly clear, and in the same situation, where about thirty-seven years before I had seen them involved in nebulosity.

If then the light of these three stars is thus proved to have undergone a visible modification in its passage through the nebulous matter, it follows that its situation among the stars is less distant from us than the largest of the three, which I suppose to be of the 8th or 9th magnitude. The farthest distance therefore, at which we can place the faintest part of the great nebula in Orion, to which the nebulosity surrounding the star belongs, cannot well exceed the region of the stars of the 7th or 8th magnitude, but may be much nearer; perhaps it may not amount to the distance of the stars of the 3d or 2nd order; and consequently the most luminous appearance of this nebula must be supposed to be still nearer to us. From the very considerable changes I have observed in the arrangement of its nebulosity, as well as from its great extent, this inference seems to have the support of observation; for in very distant objects we cannot so easily perceive changes as in near ones, on account of the smaller angles which both the objects and its changes subtend at the eye. The following memorandum was made when I viewed it in 1774; "its "shape is not like that which Dr. SMITH has delineated in his "optics, though somewhat resembling it, being nearly as in "fig. 37: from this we may infer that there are undoubtedly "changes among the regions of the fixed stars; and perhaps "from a careful observation of this lucid spot, something may "be concluded concerning the nature of it."

In January 1783, the nebulous appearance differed much from what it was in 1780, and in Septemper it had again undergone a change in its shape since January.

March 13, 1811. With a view to ascertain such obvious alterations in the disposition of the nebulous matter as may be depended on, I selected a telescope that had the same light and power which thirty-seven years ago I used, when I made the above-mentioned drawing; and the relative situation of the stars remaining as before, I found that the arrangement of the nebulosity differs considerably. The northern branch *N* still remains nearly parallel to the direction of the stars *a b*; but the southern branch *S* is no longer extended towards the star *d*; its direction is now towards *e*, which is very faintly involved in it. The figure of the branch is also different; the nebulosity in the parallel *P F* of the three stars being more advanced towards the following side than it was formerly.

I compared also the present appearance of this nebula with the delineation which HUYGHENS has given of it in his *Systema Saturnium*, page 8, of which fig. 38 is a copy. The twelve stars which he has marked are sufficient to point out the arrangement of the nebulous matter at the time of his observation. By their situation we find that the nebula had no southern branch, nor indeed any to the north, unless we call the nebulosity in the direction of the parallel a branch; but then this branch is not parallel to a line drawn from *a* to the star *b*; moreover the star *f* is now involved in faint nebulosity, which also reaches nearly up to *g*, and quite incloses *h*. The star *b* which is now nebulous, is represented as perfectly out of all nebulosity, and can hardly be supposed to have been affected when HUYGHENS observed it.

The changes that are thus proved to have already happened, prepare us for those that may be expected hereafter to take place, by the gradual condensation of the nebulous matter: for had we no where an instance of any alteration in the appearance of nebulæ, they might be looked upon as permanent celestial bodies, and the successive changes, to which by the action of an attracting principle they have been conceived to be subject, might be rejected as being unsupported by observation.

The various appearances of this nebula are so instructive, that I shall apply them to the subject of the partial opacity of the nebulous matter, which has already been inferred from its planetary appearance, when extremely condensed in globular masses; but which now may be supported by more direct arguments. For when I formerly saw three fictitious nebulous stars, it will not be contended that there were three small shining nebulosities, just in the three lines in which I saw them, of which two are now gone and only one remaining. As well might we ascribe the light surrounding a star, which is seen through a mist, to a quality of shining belonging to that particular part of the mist, which by chance happened to be situated where the star is seen. If then the former nebulosity of the two stars which have ceased to be nebulous can only be ascribed to an effect of the transit or penetration of their light through nebulous matter which deflected and scattered it, we have now a direct proof that this matter can exist in a state of opacity, and may possibly be diffused in many parts of the heavens without our being able to perceive it.

That there has been shining as well as opaque nebulous matter about the large star, appears from several observations I have made upon the light which surrounded it. In 1783 the nebulosity about it was so considerable in brightness, and so much on one side of it, that the star did not appear to have any connection with it. The reason of which is plainly, that the shining quality of the nebulous matter then overpowered the feeble scattering of the light of the star in the nebulosity.

32. *Of Stellar Nebulæ.*

It has been remarked that diffused nebulosities may exist unknown to us, among the more distant regions of the fixed stars; and though we may not be able to see a nebulous diffusion that is farther from us than the moderate distance at which we now have reason to suppose the faintest visible nebulosity of the nebula in Orion to be placed; yet if some former diffusion of the nebulous matter should be already reduced into separate and much condensed nebulæ, they might then come within the reach of telescopes that have a great power of collecting light: this being admitted, there is a probability that some of the various diffusions of the nebulous matter, from which our present nebulæ derive their origin, may have been much farther from us than others. For, in every description of figure, size and condensation, of which I have given instances, we find not only very bright and very large, but also faint and small, as well as extremely faint and extremely small nebulæ; and the same gradations will now be found to run through that class which I have called stellar nebulæ. This classification was introduced in my sweeps when the objects to be recorded came in so quick a succession that I found it expedient to express as much as I could in as few words as possible, and by calling a nebula stellar, I intended to denote that the object to which I gave this name was, in the first place as small, or almost as small, as a star; and in the next, that notwithstanding its smallness, and starlike appearance, it bore evident marks of not being one of those objects which we call stars, and of which I saw many at the same time in the telescope.

The following three collections contain one hundred and seventeen stellar nebulæ, which have been assorted by their brightness, that their comparative condensation might be estimated according to the different distances at which we may suppose other nebulæ of the same degree of light to be placed.*

I. 71 is "A considerably bright, very small, almost stellar "nebula; the brightness diminishing insensibly and breaking "off pretty abruptly. The whole together is not more than "about 7 or 8″ in diameter." A second observation, made in a "remarkable clear morning, says, that "the greatest bright-"ness is towards the following side, and that the very faint "nebulosity extends to near a minute."

This is probably a condensation of a former nucleus with surrounding chevelure.

I. 268 is "A very bright, very small, round stellar nebula." Fig. 39.

This may be a former planetary nebula in a higher state of condensation.

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* See *First assortment containing six of the brightest stellar nebulæ.* I. 71, 268. II. 110, 603. IV. 32, 46.

II. 110 is "A very bright small stellar nebula or star "with a bur all-around." Fig. 40.

This star with a bur is probably one that was formerly a planetary nebula with a pretty strong haziness on the borders.

II. 603 is "A pretty bright stellar nebula, or a pretty considerable star with a very faint chevelure." Fig. 41.

This may have been a planetary nebula with a faint haziness about the margin.

IV. 46 is "A very small pretty bright, or considerably "bright stellar nebula, like a star with burs."

It may have been a pretty well defined planetary nebula.

If it should be deemed singular that we have not a greater number of bright stellar nebulæ, I must remark that, if the stellar is a succession of the planetary state, the number of bright stellar is sufficiently proportionable to that of the planetary nebulæ; and as the faint nebulæ are far more numerous than the bright ones, so it will be seen by the references in the two next assortments, that in proportion as brightness decreases, we have a much more copious collection of stellar nebulæ.[*]

II. 663 is "A pretty bright very small stellar nebula."

This nebula and the rest of them, which are all of the same description, must be looked upon as condensations of distant nebulæ that had nuclei, or were nearly about the planetary condition.[†]

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[*] See *Second assortment containing eleven stellar nebulæ of the next degree of brightness.* II. 159, 178, 179, 204, 232, 663, 676, 689, 708, 820, 867.

[†] See *Third assortment containing one hundred stellar nebulæ of several degrees of faintness.* II. 127, 194, 244, 340, 341, 425, 443, 448, 449, 454, 550, 551, 576, 618, 620, 692, 693, 718, 721, 722, 735, 740, 781, 815, 848. III. 81, 109, 114, 119,

In this collection of nebulæ we have many of a different description. In some, the mark whereby they were distinguished from stars was their figure, the object not being so small but that its figure might still be perceived. Of others, some difference in the brightness between the center and outside was visible; and many of them were only called stellar, because by some deficiency or other in the appearance it was evident they were not perfect stars. Instances of every sort will be seen in the following descriptions.

II. 424 is "A very faint stellar nebula, or a little larger."

II. 805 is "An extremely faint very small round stellar nebula."

II. 425 is "A faint very small stellar nebula, of an irre-"gular figure."

III. 145 is "A very faint stellar nebula; a little extended."

III. 691 is "A considerably faint stellar nebula, suddenly "much brighter in the middle."

33. *Of Stellar Nebulæ nearly approaching to the Appearance of Stars.*

The starlike appearance of the following six nebulæ is so considerable that the best description, which at the time of observation I could give of them, was to compare them to stars with certain deficiencies.*

125, 136, 145, 151, 161, 167, 168, 169, 170, 171, 172, 173, 175, 188, 215, 231, 232, 234, 240, 260, 276, 277, 278, 289, 294, 320, 322, 341, 400, 401, 418, 422, 423, 424, 438, 439 469, 476, 530, 536, 561, 562 563, 564, 565, 571, 576, 590, 606, 627, 672, 691, 706, 737, 741, 764, 768, 770, 772, 777, 786, 793, 805, 815, 821, 828, 843, 852, 855, 856, 916

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* See *Three stars with burs.* II. 655. IV. 47, 49.

IV. 49 is "A pretty bright stellar nebula, like a star with a "small bur all around."

The other two are of the same nature.*

IV. 15 is "A stellar nebula, or rather like a faint star with "a small chevelure and two burs."

The other two are nearly of the same description.

34. *Of doubtful Nebulæ.*

It may have been remarked, that many stellar nebulæ of my catalogues have the memorandum added to their descriptions that they were confirmed with a higher magnifying power, and that this was sometimes attended with difficulty, and sometimes could not be successively done.

A collection of thirty-four nebulæ that come under this description is as follows: †

II. 470 is "A small stellar nebula." By a second observation a doubt entertained in the first was removed with 240, which shewed it "pretty bright, but hardly to be distinguished from a star."

III. 29 is "A very faint extremely small stellar nebula or "rather nebulous star." The sweeping power left me rather "doubtful; 240 verified it.

It must be noticed, that in these nebulæ the doubt which was entertained did not relate to the existence of the objects, but merely to their nature; and when the suspected nebula was so faint that even its existence was doubtful, a higher power was applied only with a view to ascertain whether the object existed as nebula or as star; for had the suspicion of its existence not been accompanied with the expectation of its being a nebula, it could never have been attempted to be verified.*

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* See *Three stars with a faint chevelure.* IV. 15, 21, 31.

† See *First assortment containing twenty-five verified stellar nebulæ.* II. 470, 502, 661. III. 29, 80, 84, 124, 155, 174, 184, 187, 202, 207, 214, 226, 204 266, 268, 269, 513, 549, 604, 742, 748, 964.

III. 270 is "A very faint extremely small stellar nebula; "240 verified it with difficulty, and considerable attention, the "night being uncommonly clear."

When difficulty is mentioned, it is always to be understood that a considerable time as well as attention was required in the examination before a decisive opinion could be formed.†

III. 7 is "A nebulous star, but doubtful of the nebulosity. "With 240 the same doubtful appearance continues." Fig. 42.

With this object the doubt which remained could only relate to the nature of it; for being at first sight taken to be a nebulous star, its existence could not be a subject for examination; but the unresolved doubt, whether an object is a nebula or a star, must certainly be allowed to be as great a proof of identity as we can possibly expect to see.

35. *Concluding Remarks.*

The total dissimilitude between the appearance of a diffusion of the nebulous matter and of a star, is so striking, that an idea of the conversion of the one into the other can hardly occur to any one who has not before him the result of the critical examination of the nebulous system which has been displayed in this paper. The end I have had in view, by arranging my observations in the order in which they have been placed, has been to shew, that the above mentioned extremes may be connected by such nearly allied intermediate steps, as will make it highly probable that every succeeding state of the nebulous matter is the result of the action of gravitation upon it while in a foregoing one, and by such steps the successive condensation of it has been brought up to the planetary condition. From this the transit to the stellar form, it has been shown, requires but a very small additional compression of the nebulous matter, and several instances have been given which connect the planetary to the stellar appearance.

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* See *Second assortment, containing five stellar nebulæ verified with difficulty.* III. 115, 212, 219, 262, 270.

† See *Third assortment, containing four objects that could not be verified.* III. 7, 176, 263, 293.

The faint stellar nebulæ have also been well connected with all sorts of faint nebulæ of a larger size; and in a number of the smaller sort, their approach to the starry appearance is so advanced, that in my observations of many of them it became doubtful whether they were not stars already.

It must have been noticed, that I have confined myself in every one of the preceding articles to a few remarks upon the appearance of the nebulous matter in the state in which my observations represented it; they seemed to be the natural result of the observations under consideration, and were not given with a view to establish a systematic opinion, such as will admit of complete demonstration. The observations themselves are arranged so conveniently that any astronomer, chemist, or philosopher, after having considered my critical remarks, may form what judgment appears most probable to him. At all events, the subject is of such a nature as cannot fail to attract the notice of every inquisitive mind to a contemplation of the stupendous construction of the heavens; and what I have said may at least serve to throw some new light upon the organization of the celestial bodies.

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POSTSCRIPT.

IT will be seen that in this paper I have only considered the nebulous part of the construction of the heavens, and have taken a star for the limit of my researches. The rich collection of clusters of stars contained in the 6th, 7th, and 8th classes of my Catalogues, and many of the *Connoissance des Temps*, have as yet been left unnoticed. Several other objects, in which stars and nebulosity are mixed, such as nebulous stars, nebulæ containing stars, or suspected clusters of stars which yet may be nebulæ, have not been introduced, as they appeared to belong to the sidereal part of the construction of the heavens, into a critical examination of which it was not my intention to enter in this Paper.

WILLIAM HERSCHEL.

Slough, near Windsor,
May 26, 1811.

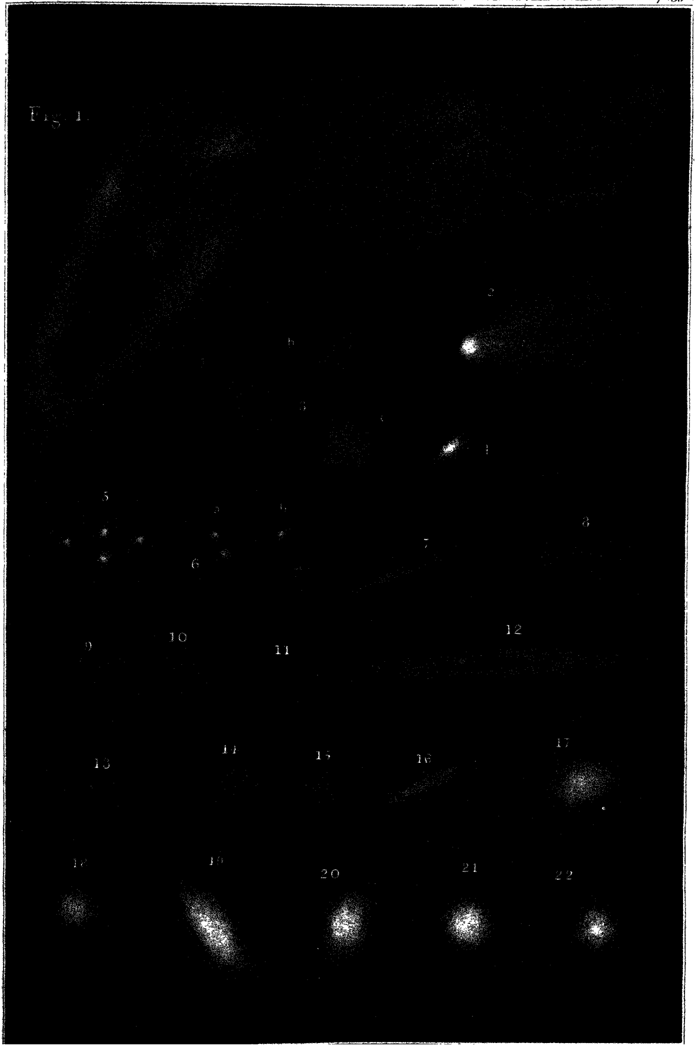
Fig. 1.
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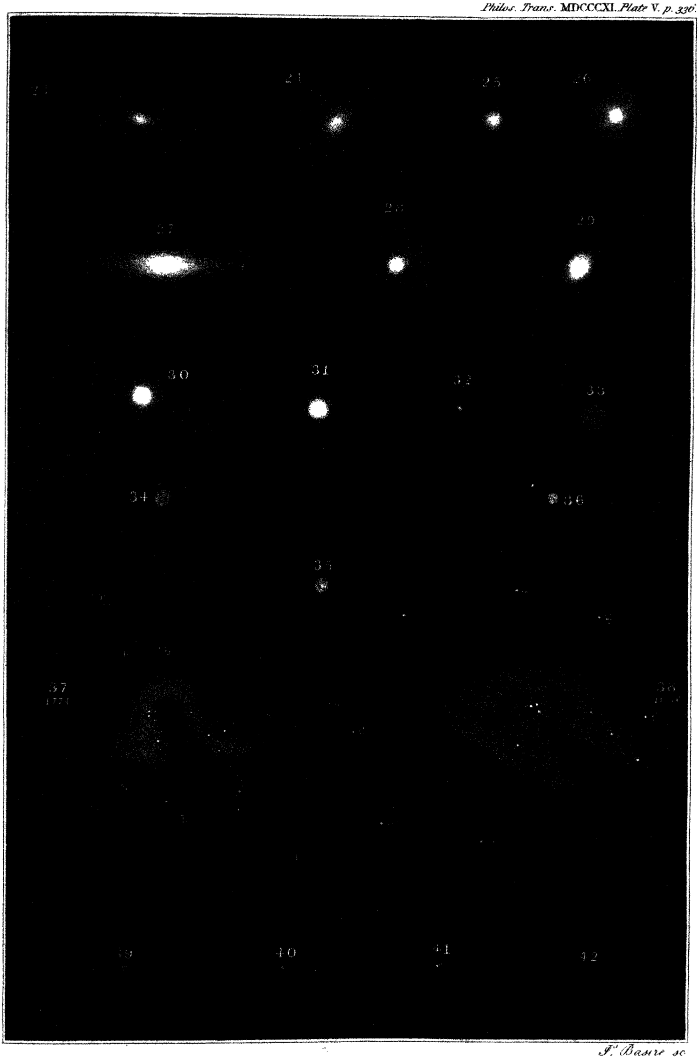

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XVII. *Experiments to ascertain the State in which Spirit exists in fermented Liquors: with a Table exhibiting the relative Proportion of pure Alcohol contained in several Kinds of Wine and some other Liquors. By* William Thomas Brande, *Esq. F. R. S.*

Read June 13, 1811.

SECTION I.

It has been a commonly received opinion, that the alcohol obtained by the distillation of wine, does not exist ready formed in the liquor, but that it is principally a product of the operation, arising out of a new arrangement of its ultimate elements.

The proofs which have been brought forward in support of this theory, are chiefly founded on the researches of Fabroni,* who attempted to separate alcohol by saturating the wine with dry subcarbonate of potash, but did not succeed, although by the same means he could detect very minute portions of alcohol which had been purposely added.

To obtain satisfactory results from many of the following experiments, it became necessary to employ wines to which little or no spirit had been added; for a very considerable addition of brandy is made to most of the common wines, even before they are imported into this country. I therefore occasionally used Burgundy, Hermitage, Cote Roti, Champagne, Frontignac, and some other French wines, to which, when of the best quality, no spirit can be added, as even the smallest proportion impairs the delicacy of their flavour, and is consequently readily detected by those who are accustomed to taste them. For these, and for the opportunity of examining many of the scarce wines enumerated in the table annexed to this Paper, I am indebted to the liberality of the Right Hon. Sir JOSEPH BANKS.

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* Annales de Chimie, XXXI. p. 303.

Dr. BAILLIE, who took considerable interest in this investigation, was also kind enough to procure for me some port wine, sent from Portugal for the express purpose of ascertaining how long it would remain sound, without any addition whatever of spirit having been made to it.

Lastly, I employed raisin wine, which had been fermented without the addition of spirit.

At a very early period of the present inquiry, I ascertained by the following experiments, that the separation of the alcohol, by means of subcarbonate of potash, was interfered with, and often wholly prevented by some of the other ingredients of the wine.

A pint of port wine was put into a retort placed in a sand heat, and eight fluid ounces were distilled over, which by saturation with dry subcarbonate of potash, afforded about three fluid ounces of tolerably pure spirit floating on the surface.

I repeated this distillation precisely under the same circumstances, and mixed the distilled liquor with the residuum in the retort, conceiving that if the spirit were a product, I now should have no difficulty in separating it from the wine by the addition of subcarbonate of potash; but although every precaution was taken, no spirit separated: a portion of the subcarbonate, in combination with some of the ingredients of the wine, formed a gelatinous compound, and thus prevented the appearance of the alcohol.

It has been remarked by Fabroni, in the Memoir above quoted, that one hundredth part of alcohol purposely added to wine may be separated by subcarbonate of potash, but several repetitions of the experiment have not enabled me to verify this result; when however a considerable addition of alcohol has been made to the wine, a part of it may be again obtained by saturation with the subcarbonate. The necessary addition of spirit to port wine, for this purpose, will be seen by the following experiments.

Four ounces of dry and warm subcarbonate of potash were added to eight fluid ounces of port wine, which was previously ascertained to afford by distillization 20 per cent. of alcohol (by measure), of the specific gravity of 0,825 at 60°.

In twenty-four hours the mixture had separated into two distinct portions; at the bottom of the vessel was a strong solution of the subcarbonate, upon which floated a gelatinous substance, of such consistency as to prevent the escape of the liquor beneath when the vessel was inverted, and which appeared to contain the alcohol of the wine, with the principal part of the extract, tan, and colouring matter, some of the subcarbonate, and a portion of water; but as these experiments relate chiefly to the spirit contained in wine, the other ingredients were not minutely examined.

To seven fluid ounces of the same wine, I added one fluid ounce of alcohol (specific gravity 0,825), and the same quantity of the subcarbonate of potash as in the last experiment;

but after twenty-four hours had elapsed, no distinct separation of alcohol had taken place.

When two fluid ounces of alcohol were added to six fluid ounces of the wine, and the mixture allowed to remain undisturbed for the same length of time as in the former experiments, a stratum of impure alcohol, of about a quarter of an inch in thickness, separated on the surface.

The addition of three fluid ounces of the alcohol to five fluid ounces of the wine, formed a mixture from which a quantity of spirit readily separated on the surface, when the subcarbonate was added, and the gelatinous compound sunk nearly to the bottom of the vessel, there being below it a strong solution of the subcarbonate.

When in these experiments Madeira and Sherry were employed instead of Port wine, the results were nearly similar.

It was suggested to me by Dr. Wollaston, that if the wine were previously deprived of its acid, the subsequent separation of the alcohol, by means of potash, might be less interfered with. I therefore added, to eight fluid ounces of port wine, a sufficient quantity of carbonate of lime to saturate the acid, and separated the insoluble compounds produced by means of a filter. The addition of potash rendered the filtered liquor turbid, some soluble salt of lime, probably the malate, having passed through the paper; but the separation of alcohol was as indistinct, as in the experiments just related.

It is commonly stated, that the addition of lime water to wine, not only forms insoluble compounds with the acids, but also with the colouring matter, and that these ingredients may be thus separated without heat; but on repeating these experiments, they did not succeed, nor could I devise any mode of perfectly separating the acids, and the extractive and colouring matter (excepting by distillation), which did not interfere with the alcohol.

If the spirit afforded by the distillation of wine were a *product* and not an *educt*, I conceived that by performing the distillation at different temperatures, different proportions of spirit should be obtained.

The following are the experiments made to ascertain this point.

Four ounces of dried muriate of lime were dissolved in eight fluid ounces of the Port wine employed in the former experiments: by this addition, the boiling point of the wine, which was 190° FAHRENHEIT, was raised to 200°. The solution was put into a retort placed in a sand heat, and was kept boiling until four fluid ounces had passed over into the receiver, the specific gravity of which was 0,96316 at 60° FAHRENHEIT.*

The experiment was repeated with eight fluid ounces of the wine without any addition, and the same quantity was distilled over, as in the last experiment: its specific gravity at 60° FAHRENHEIT, was 0,96311.

Eight fluid ounces of the wine were distilled in a water bath: when four fluid ounces had passed over, the heat was withdrawn. The specific gravity of the liquor in the receiver was 0,96320 at 60° FAHRENHEIT.

The same quantity of the wine, as in the last experiment, was distilled at a temperature not exceeding 180° FAHRENHEIT. This temperature was kept up from four to five hours, for five successive days, at the end of which period, four ounces having passed into the receiver; its specific gravity at 60° was ascertained to be 0,96314.

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* It was supposed that in this experiment a small portion of muriate of lime might have passed over into the receiver, but the distilled liquor did not afford the slightest traces of it, to the tests of oxalate of ammonia and nitrate of silver.

It may be concluded, from these results, that the proportion of alcohol is not influenced by the temperature at which wine is distilled, the variation of the specific gravities in the above experiments being even less than might have been expected, when the delicacy of the operation by which they are ascertained, is considered.

I have repeatedly endeavoured to separate the spirit from wine, by subjecting it to low temperatures, with a view to freeze the aqueous part; but when the temperature is sufficiently reduced, the whole of the wine forms a spongy cake of ice.

In a mixture of one fluid ounce of alcohol with three of water, I dissolved the residuary matter, afforded by evaporating four fluid ounces of Port wine, and attempted to separate the alcohol from this artificial mixture by freezing; but a spongy cake of ice was produced as in the last experiment.

When the temperature is more gradually reduced, and when large quantities of wine are operated upon, the separation of alcohol succeeds to a certain extent, and the portion which first freezes is principally, if not entirely water, hence in some countries this method is employed to render wine strong.

SECTION II.

Having ascertained that alcohol exists in wine ready formed, and that it is not produced during distillation, I employed that process to discover the relative proportion of alcohol contained in different wines.

In the following experiments, the wine was distilled in glass retorts, and the escape of any uncondensed vapour was prevented by employing sufficiently capacious receivers, well luted, and kept cold during the experiment.

By a proper management of the heat towards the end of the process, I could distil over nearly the whole of the wine without burning the residuary matter: thus, from a pint of Port wine, of Madeira, of Sherry, &c. I distilled off from fifteen fluid ounces, to fifteen fluid ounces and a half; and from the same quantity of Malaga, and other wines containing much saccharine matter, I could readily distil from fourteen to fifteen fluid ounces.

In order to ascertain the proportion of alcohol with precision, pure water was added to the distilled wine, so as nearly to make up the original measure of the wine, a very small allowance being made for the space occupied by the solid ingredients of the wine, and for the inevitable loss during the experiments: thus, five fluid drachms and a half of distilled water were added to fifteen fluid ounces and a quarter of the liquor procured by the distillation of a pint of port wine, and in other cases nearly the same proportions were observed. This mixture of the distilled wine and water, was immediately transferred into a well stopped phial, and having been thoroughly agitated, was allowed to remain at rest for some hours; its specific gravity (at the temperature of 60° FAHRENHEIT), was then very carefully ascertained, by weighing it in a bottle holding exactly one thousand grains of distilled water at the above temperature, and the proportion of alcohol per cent. *by measure*, was estimated by a reference to Mr. GILPIN'S tables,* the specific gravity of the standard alcohol being 0,82500 at 60°.

As the most convenient mode of exhibiting the results of these numerous experiments, I have thrown them into the form of a table; in the first column the wine is specified; the second contains its specific gravity after distillation, as above described; and the third exhibits the proportion of the pure spirit, which every hundred parts of the wine contain. I have also inserted porter, ale, cyder,† brandy, and some other spirituous liquors, for the convenience of comparing their strength, with that of the wines.

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* Phil. Trans. 1794.

† The proportion of spirit, which may be obtained from these three liquors, is subject to considerable variation in different samples: the number given for each, in the table, is therefore the mean of several experiments, as it did not seem necessary to specify them separately.

Wine.	Specific Gravity after Distillation.	Proportion of Alcohol, per Cent. by Measure.
Port - -	0,97616	21,40
Ditto -	0,97532	22,30
Ditto - -	0.97430	23,39
Ditto -	0,97400	23,71
Ditto - -	0,97346	24,29
Ditto -	0,97200	25,83
Madeira - -	0,97810	19,34
Ditto - -	0,97616	21,40
Ditto - -	0,97380	23,93
Ditto -	0,97333	24,42
Sherry - -	0,97913	18,25
Ditto - -	0,97862	18,79
Ditto -	0,97765	19,81
Ditto - -	0,97700	19,83
Claret -	0,98440	12,91
Ditto - -	0,98320	14,08
Ditto -	0,98092	16,32
Calcavella -	0,97920	18,10
Lisbon - -	0,97846	18,94
Malaga -	0,98000	17,26
Bucellas - -	0,97890	18,49
Red Madeira -	0,97899	18,40
Malmsey Madeira	0,98090	16,40
Marsala - -	0,97196	25,87
Ditto - -	0,98000	17,26
Red Champagne -	0,98608	11,30
White Champagne	0,98450	12,80
Burgundy -	0,98300	14,53
Ditto - -	0,98540	11, 5
White Hermitage	0,97990	17,43
Red Hermitage -	0,98495	12,32
Hock - -	0,98290	14,37
Ditto - -	0,98873	8,88
Vin de Grave -	0,98450	12,80

Wine.	Specific Gravity after Distillation.	Proportion of Alcohol, per Cent. by Measure.
Frontignac	0,98452	12,79
Cote Roti	0,98495	12,32
Rousillon	0,98005	17,26
Cape Madeira	0,97924	18,11
Cape Muschat	0,97913	18,25
Constantia	0,97770	19·75
Tent	0,98399	13,30
Sheraaz	0,98176	15,52
Syracuse	0,98200	15,28
Nice	0,98263	14,63
Tokay	0,98760	9,88
Raisin Wine	0,97205	25,77
Grape Wine	0,97925	18,11
Currant Wine	0,97696	20,55
Gooseberry Wine	0,98550	11,84
Elder Wine	0,98760	9,87
Cyder	0,98760	9,87
Perry	0,98760	9,87
Brown Stout	0.99116	6,80
Ale	0,98873	8,88
Brandy	0.93544	53,39
Rum	0,93494	53,68
Hollands	0,93855	51,60

XVIII. *Account of a Lithological Survey of Schehallien, made in order to determine the specific Gravity of the Rocks which compose that Mountain.* *By* John Playfair, *Esq. F. R. S.*

Read June 27, 1811.

THE astronomical observations made on the mountain Schehallien, in 1774, were confessedly of great importance to science. They ascertained the power of mountains to produce a sensible disturbance in the direction of the plumb-line; of consequence, they proved the general diffusion of gravity through terrestrial substances, and afforded data for determining the medium density of the earth, compared with that of the bodies at its surface.

The skill with which this very delicate experiment was conducted by Dr. MASKELYNE, and the ingenuity with which the results were deduced by Dr. HUTTON, were worthy of the objects in view, and of the reputation which these distinguished men have acquired in their respective departments of the mathematical sciences.

One thing only seemed wanting to give to the determination of the earth's density all the accuracy that could be obtained from a single experiment, namely, a more precise knowledge of the specific gravity of the rock which composes the mountain, as being the object with which the mean density of the earth was immediately compared. The specific gravity of that rock was assumed to be to that of water as 5 to 2;

which, though it be nearly a medium when stones of every kind, from the lightest to the heaviest, are included, is certainly too small for Schehallien, the rocks of which belong to a class of a specific gravity considerably above the mean. The uncertainty arising from this source might not be of great amount, yet it was desirable that the quantity, or, at least, the limits of it should be accurately ascertained. In this light I knew, from repeated conversations, that the matter was regarded by both the gentlemen above named.

I had therefore long wished to attempt such a survey of the mountain as might afford a satisfactory solution of this difficulty; and having mentioned the circumstances to the Right Hon. Lord WEBB SEYMOUR, he entered readily into a scheme, which without the assistance of his skill and activity, I should have been quite unable to carry into execution.

Accordingly, in June 1801, we took up our residence in a small village as near as we could to the bottom of the mountain, and began our Mineralogical Survey, the result of which we think it our duty to submit to the Society, under the auspices of which the original experiment was undertaken.

It was obvious, that our first object must be to obtain specimens of all the varieties of rock in the mountain, which had any considerable difference in their external characters. These specimens must be such as had not been exposed to the action of the weather, were perfectly sound, with a fresh fracture, and taken from the living rock. In order to procure these, we soon found that it was not necessary to dig into the mountain or to blast the stones with gun-powder, for the native rock breaks out on the bare and rugged surface in abundance of places, and is so deeply intersected by the streams that it was easy, by the assistance of the hammer only, to procure specimens having all the conditions requisite for our purpose.

Supposing, however, that all this was accomplished, it would be insufficient to determine the mean density of the mountain, unless the quantity of rock of each particular kind could also be estimated; at least nearly. It was necessary, therefore, to know what proportion of the mountain consisted of one species of rock, and what of another, without which the average could not be determined.

Had the mean density been the only thing wanted, it would have been sufficient to know the quantity of each variety of rock; but in the search we were engaged in, it was necessary to know not only the quantity, but the position of each of these varieties, relatively to the observatories on the south and north faces of the mountain. This will be evident, when it is considered that it was the effect of each portion of the rock on the plumb-line in these observatories that was the thing to be found, and that this effect must vary not only with the density of the rock, but with its distance from the observatory, and its obliquity in respect of the meridian. The mean density would therefore be insufficient for estimating the attraction of the mountain, could it be found ever so exactly; and it is easy to shew, that while the mean density of a heterogeneous mass, and also its magnitude and figure remain the same, its attractive force at a given point may be greatly changed by a different distribution of the materials it consists of, relatively to that point. In order then to form an estimate of this attraction we must know, at least nearly, these three things, the varieties of rock composing the mountain; the quantity of each variety; and, lastly, the position of each relatively to the observatory. Fortunately the Geometrical Survey of the mountain, which had already determined not merely its superficial extent but its solidity, taken in combination, with some peculiarities in its structure, have enabled us to approximate, I hope with some tolerable exactness, to the knowledge of all these three circumstances.

The plan, then, which we proposed to follow, and which was necessary to be pursued if our Lithological Survey was to correspond in any degree to the accuracy of the Geometrical Survey, made under the direction of the Astronomer Royal, was to try to recognise the chain of stations which had been employed in that survey, in order that, by reference to those stations, we might be able to determine the points on the surface of the mountain from which our different specimens were collected. After these stations were discovered, we meant to traverse the mountain in various directions, and at any point where a specimen was taken, to determine our position by the bearings of any two of the stations that might be in sight, or by taking angles to three of them, or such other methods as occasion and circumstances might suggest. This was to be done where considerable variations in the external characters of the rocks gave reason to look for considerable variations of specific gravity. It was an operation that could not be necessary for every individual specimen, but it was one which must be necessary for determining the district over which stone of a particular character prevailed. In this part of the work we were to employ a theodolite, a sextant or a compass, according as more or less accuracy seemed requisite.

As the marks of the stations were all effaced except some traces of the observatories (or rather the huts in which Dr. MASKELYNE had lived), and the two cairns on the top of the mountain, the discovery of the whole chain was a matter of some difficulty. By means, however, of the bearings, as given in Dr. HUTTON's paper, and the assistance of one of the guides who had been employed about the survey, we succeeded in finding out the stations; and as they were mostly on elevated points, we could distinguish them at a distance with sufficient exactness.

Schehallien belongs to one of the central ridges of the Grampians, which, stretching here from about SE. to NW. divides the vallies of the Tummel and the Tay. Though it be a part of this chain, it stands considerably separate from the rest on a base of a form somewhat oval, and having its figure distinctly defined by two streams that run, the one on the south, and the other on the north side of it. The lowest point in this base, which is on the NE. is 2467 feet below the summit of the mountain, and about 1094 above the level of the sea.

At the NW. extremity, Schehallien adheres to the main chain by means of a high ridge, depressed at its lowest point little more than 1500 feet under the summit. On the opposite sides of this neck the streams rise, which were before said to determine the base of the mountain; these streams, however, do not unite at the eastern extremity of the base, for there also a sort of neck, though very low in comparison of the former, connects Schehallien with the hills to the eastward.

Beyond the streams just mentioned, a range of inferior hills, some of them very low, springing from the main ridge on the NW. encompasses the mountain, forming as it were a line of circumvallation round it, and on these were the stations which Mr. BURROWS, under Dr. MASKELYNE'S direction, had chosen for the survey. Beyond these hills the ground falls down into a sort of plain of great extent on the north; on the south, less considerable and more uneven, yet such as to leave Schehallien very free and open in the direction of the meridian, and adapted by that means to shew the full amount of its action on the plummet. From the base its sides rise with a rapid, though unequal acclivity, and terminate not in a point, but in a ridge or narrow plane of a waving form, about a mile in length, and sloping regularly to the east, where it is 480 feet lower than at the western extremity. Though the sides are very rugged, they are less broken by deep ravines or bold projections than the other mountains of the same elevation in this quarter of the Grampians; for, beside the high neck which has been already mentioned as uniting Schehallien to the mountains on the west it has only one other saliant ridge, which runs out to the NE. and overlooks the plain with a very steep and precipitous aspect. In some directions, and when viewed from a considerable distance, the harsh features of the mountain are wonderfully softened; it acquires a very beautiful conoidal shape, and from thence derives the name by which it is known among the inhabitants of the low country.

The rock of Schehallien, like that of all the mountains in its vicinity, is of the class called primitive; and is disposed for most part in great parallel plates, or strata, nearly vertical, stretching from SE. to NW. They are indeed so nearly vertical, that a deviation of 15° from the perpendicular is rarely to be met with, except toward the base of the mountain, where it is sometimes greater, and is subject to considerable inequalities. The strata on the north side of the mountain lean a little toward the north, and those on the south toward the south. All these variations, however, are inconsiderable, and in general the strata may be set down as nearly vertical.

But though in their disposition all the rocks of Schehallien agree pretty nearly, they differ considerably in their mineralogical characters. A large proportion of the mountain, and that which constitutes the most elevated part, is formed of a granular quartz, extremely hard, compact, and homogeneous. The whole mass from about the level of the two observatories up to the summit of the mountain, is of this stone. Lower down, again, on every side, the rock is a schistus containing much mica and hornblend; and the division into parallel and vertical plates is more obvious than in the granular quartz. This last, however, is sometimes found in the lower parts, forming thin, vertical plates, interstratified with the hornblend and mica slate, and all together preserving their parallelism with a neatness and accuracy which a work of art could hardly exceed. This is particularly to be observed in the bed of the *burn* of Glenmore, the stream that defines the base of the mountain on the south, and which toward the lower part of its course intersects the strata to a great depth.

Besides these two kinds of rock, we meet in several places toward the base of the mountain with a granular and micaceous limestone highly cristallized, which in one or two places ascends to a considerable height. All these rocks are disposed in strata; but there are also veins or dykes of porphyry and greenstein, which traverse the mountain in different directions;

one of the former kind, of great breadth, cuts it right across from NE. to SW. not far from the point of its greatest elevation. There is no where any appearance of metallic veins.

The quartzy rock of Schehallien is extremely hard and homogeneous, and by its slow and uniform decay, has no doubt given rise to that massive shape, and comparatively unbroken surface, which have been already remarked. Yet even here the work of time is abundantly evident; for the rock being much cut by fissures transverse to its stratification, it separates and falls down in large prismatic fragments. Some of these are of a vast size, and being extremely durable, the accumulation of them where the ground is not too steep to permit them to lie, is very great, so that large tracts of the sides of the mountain are covered with cubical blocks of granular quartz, resting on one another, and *steadied* only by their own weight.

It is remarkable of this quartzy stone, that when exposed for some time to the weather, it acquires the lustre and appearance of white enamel, so that the old weather beaten surface is more clear and shining than that which is immediately produced from a fresh fracture. The reason seems to be, that the stone does not consist of pure quartz, but along with the grains of quartz has a great number of grains of felspar interspersed, which when it is first broken give it an opaque and earthy appearance. These are soon dissolved by the action of the weather; and there is then left over the surface a coat of pure quartz, which has the semi-transparency and vitreous gloss belonging to enamel.

The felspar which enters into the composition of the rock here described, is not always in grains, but in some specimens is found regularly crystallized. The crystals however are small and very thinly disseminated; were they in more abundance this stone might be accounted a granite, as Professor JAMESON has remarked of a stone of the same kind which he found in the island of Jura.

From the vertical position of the strata we may infer with some probability, that the rock which breaks out any where at the surface, continues the same through the interior of the mountain, in the direction of a perpendicular plane, down to its base, or perhaps to an indefinite depth. The same stratum usually remains of the same nature to a great extent, whenever we have an opportunity of examining it, whether in a horizontal or a perpendicular direction; and it is not to be doubted that the same holds when no such opportunity occurs. When, therefore, we have on the surface a bed of mica slate, or of granular limestone, or of granular quartz, the probability is that the whole stratum all through the mountain is composed of the same materials. I must however confess, that I do not think that this probability is as strong with respect to granular quartz, as it is with respect to the micaceous rocks. These last compose the great mass of the Grampians; and their characters, though not every where the same, change very slowly, and pass from one to another by imperceptible gradations. To the granular quartz this rule does not equally apply; it is not general among the mountains of this tract; it sometimes breaks off suddenly, and is replaced by rocks of a very different nature. We cannot therefore with the same confidence assume the existence of this rock in intermediate points, when we only see it in the extremes. This much, however, we know with certainty, that the whole of the upper part of the mountain, from about the level of the south observatory to the summit, consists of granular quartz, as no other stone is to be met with any where in that tract. This is the part above O, in the section of the mountain; and the only question is, whether we shall consider the part in the interior of the mountain, immediately under this mass, as consisting of the same rock. When we first examined Schehallien, Lord. WEBB SEYMOUR and I were both of opinion that this was the most probable supposition. Since that time, however, having had an opportunity of examining some other of the Grampians where granular quartz is found at the summit, and where, nevertheless, it is certain that the same rock does not go down into the interior, there has appeared some reason to suspect that this may be true of Schehallien. As the result of the calculus with regard to the earth's density is materially affected by these suppositions, I have given the result as I had first deduced it on the hypothesis, that the interior of the mountain is of granular quartz; and also on the hypothesis that the quartz is confined to the upper part; and that the lower part is entirely composed of mica and hornblend slate.

In the computation which Dr. HUTTON made of the attraction of Schehallien, he supposed its mass divided into 960 vertical columns, and he computed the force with which each of these columns disturbed the direction of a plummet suspended in either observatory, supposing them all homogeneous and two and a half times as dense as water * Now knowing from our survey, and the combination of geometrical with mineralogical observations, the specific gravity of each of these columns at the surface, and conceiving (what we have shown, with one

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* Phil. Trans. Vol. LXVIII. (1778), p. 689, &c.

exception, to be probable) that the column remains the same through its whole length, we can compare the real attraction with that assigned to it in Dr. HUTTON's calculation. The attraction of any column computed on his hypothesis being divided by 2.5, and multiplied by the true specific gravity, will give the real attraction, or effect in disturbing the plumb-line. It is on this principle that our correction is formed, though simplifications occurred that very much diminished the labour of the computation, the nature of the rock leading us in the end to distinguish only two differences of specific gravity; and the ingenious deductions of Dr. HUTTON, together with the excellent order that prevails in his computation, having made it easy to follow a route which he had cleared of all its greatest difficulties.

However, as it was impossible to determine before hand how much the specific gravity of these rocks might differ, it was necessary to conduct the survey so that every individual column, had it been necessary, might have had its specific gravity defined. For this purpose the mountain was traversed in various directions, and the points at which a transition was made from rocks of one character to those of another were carefully noted, and their position ascertained. In selecting the specimens which were to represent the rocks of the several districts into which the mountain thus became divided, attention was paid both to the prevailing stone, and to that which was least common, in order that we might, if possible, get possession both of the mean and the extremes. This was in general the principle that guided our choice of specimens, but the application of it in detail to particular instances does not admit of being explained. The reasons in every such case that determined us to take one stone and reject another, could only be perceived by an observer on the spot, whose eye was accustomed to judge of the varieties, the plenty or the rarity of the minerals that passed in review before him, by indications which it is impossible to describe in words We are here therefore with reluctance compelled to request from the reader more credit than we are able to prove to him that we deserve. We know that in doing this we are craving an indulgence which no wise and candid observer ever wished to possess; we sincerely regret that the nature of the subject forces us to make this demand, and that the part of our work which it was most difficult to perform to our own satisfaction, is quite incapable of being explained to the satisfaction of others.

Catalogue of Specimens from Schehallien.

The rocks of the mountain may be divided, as already remarked, into three classes: granular quartz; mica, and hornblend slate; granular limestone. The specific gravities were ascertained by the late Dr. KENNEDY, and it is therefore unnecessary to add that their accuracy may be perfectly relied on. The pieces weighed were between 1000 and 4000 grains: most commonly between 2000 and 3000. Different pieces of the same specimen were often examined. The water used was distilled, and always of a temperature between 60 and 61 degrees.

Quartz.

1. Gray sandstone containing mica in thin layers. Specific gravity = 2.6435.

2. White quartz, very pure. Fracture vitreous. Occurs in

beds chiefly on the NE. side of the mountain. Specific gravity = 2.6437.

3. Quartzy sandstone of a whitish gray colour with thin layers of mica. Specific gravity = 2.6296.

4. Quartzy sandstone. White colour with layers of mica. Much indurated. Somewhat ferruginous. Specific gravity = 2.65367.

5. Indurated sandstone with spiculæ of mica interspersed. Specific gravity = 2.6460.

6. Sandstone much indurated, vitreous shine, interspersed with mica. Specific gravity = 2.6269.

7. Granular quartz from near the summit; contains grains of felspar. Specific gravity = 2.6274.

8. Granular quartz; nearly the same with the preceding. Specific gravity = 2.6109.

9. Sandstone fine grained, slightly marked with iron veins. Specific gravity = 2.6296.

10. Sandstone fine grained, more indurated than the preceding. Specific gravity = 2.6576.

11. Sandstone containing calcareous matter. Specific gravity = 2.6656.

12. Granular quartz, very compact and indurated, but of a stratified structure; a little mica in thin plates. From a mean of several. Specific gravity = 2.6452.

13. Granular quartz of a flesh colour; imperfect crystals of felspar thinly disseminated. This specimen from near the top. From a mean of several peices. Specific gravity = 2.6387.

The mean of these thirteen specimens gives 2.6398 for the upper or quartzy part of the mountain.

Mica and Hornblend Slate.

1. Hornblend slate very compact. Specific gravity = 3.0642.

2. Micaceous schist, with hornblend and a small mixture of quartz. Specific gravity = 2.9385.

3. Black micaceous schistus, fine grained containing hornblend. Specific gravity = 3.0476.

4. Micaceous schistus containing pyrites and quartz in fine grains. Specific gravity = 2.7293.

5. Micaceous schistus tinged with an oxid of iron. Specific gravity = 2.7935.

6. Micaceous schist with thin plates of mica and hornblend transverse to the stratification. Specific gravity = 2.7907.

7. Micaceous schist with quartz in small grains. Specific gravity = 2.7499.

8. Another specimen nearly the same. Specific gravity = 2.7728.

9. Compact micaceous schistus, grains of felspar and quartz intermixed. Specific gravity = 2.71845.

10. Nearly the same with the preceding. Specific gravity = 2.7206.

The medium specific gravity of these ten specimens is 2.83255.

Limestone.

1. Granular limestone of a gray colour, containing some mica. Specific gravity = 2.7087.

2. Granular limestone, silver coloured, stratified structure. Specific gravity = 2.8890.

3. The same, bluish, highly crystallised. Specific gravity = 2.76057.

4. The same, finer grained, containing thin layers of mica. Specific gravity = 2.7419.

5. The same, gray coloured, and the crystals larger. Specific gravity = 2.7302.

The mean specific gravity of these five specimens is = 2.76607.

Gran. Quartz.		Mic. and Calc. Schist.		
Numbers.	Specific Gravities.	Specific Gravities.	Numbers.	
1	2.6435	3.0642	1	Mic.
2	2.6437	2.9385	2	
3	2.6296	3.0476	3	
4	2.65367	2.7293	4	
5	2.6460	2.7935	5	
6	2.6269	2.7907	6	
7	2.6274	2.7499	7	
8	2.6109	2.7728	8	
9	2.6296	2.71845	9	
10	2.6576	2.7206	10	
11	2.6656	2.7087	1	Calc.
12	2.6452	2.8890	2	
13	2.6387	2.76057	3	
Mean....	2.639876	2.7419	4	
		2.7202	5	
		2.81039	....Mean	

From the inspection of the preceding table, it is evident that the specimens relatively to their specific gravity may be divided into two classes sufficiently distinct from one another. The specimens of granular quartz are in specific gravity comprehended between 2.61 and 2.66, nearly, and the mean is 2.639876. The micaceous rocks, including the calcareous, are contained between the limit 2.7 and 3.06, the mean of all the 15 specimens being 2.81039. Now it happens fortunately, that these two classes of rocks distinguished by their specific gravity are also distinguished by their position, so that the line which separates them can be accurately traced out on the face of the mountain. As to the arrangement of the same two classes of rock in the interior of the mountain, there are only two different suppositions, as already observed, which possess any degree of probability, and the result of each is hereafter to be given. The curve line in the plan of the mountain divides the quartzy from the micaceous rocks.

I shall now proceed to state the principles on which the present investigation is founded, and the result to which it has led.

According to Dr. HUTTON's construction, if O (Fig. 1.) be the place of the plummet in the south observatory, ON the direction of the meridian, and if with a radius ON = 13333 feet, or $\frac{40000}{3}$, a quadrant of a circle be described, viz. WRN; if ON be divided into 20 equal parts, and if from O as a centrè, through each of these points of division, circles be described: lastly, if through O, radii as OH, OG, &c. be drawn such that the sine of the angle which each of them makes with the meridian shall differ from the sine of that which the contiguous radius makes with the meridian by $\frac{1}{12}$ of the radius; that is, if sine GON — sine HON. $=\frac{1}{12}$, &c. then shall every one of the twenty concentric rings be divided into twelve spaces, upon each of which if columns of homogeneous matter be supposed to stand, and to be of such altitudes as to subtend equal angles from O, the attraction of each column on the plummet at O, in the direction of the meridian ON, will be the same.

The attraction of any of these columns, as of that which stands on the base GHKL, is measured thus. Let $b =$ GL, the breadth of the column in the direction of the radius, $= \frac{40000}{3.12} = 666.666$ feet; $d =$ difference between the sines of the angles of azimuth, or sin. GON — sin. HON $= d$; E $=$ angle of elevation of the column above O: then the attraction $= b\,d \times$ sin. E.*

I have also used a theorem in these computations, which gives an accurate value of the attraction of a half cylinder of any altitude a, and any radius r, on a point in the centre of its base, and in the direction of a line bisecting the base. Let A be equal to that attraction; then A $= 2\,a$ Log. $\frac{r + \sqrt{a^2 + r^2}}{a}$, or A $= 2\,a$ Log. $\frac{r}{a}\left(1 + \sqrt{1 + \frac{a^2}{r^2}}\right)$.

Fig. 2. represents a vertical section of Schehallien in the direction of the meridian of the south observatory O.† The line QR represents the level of the lowest part of the base of the mountain. P is the north observatory; the part of the section coloured with a reddish brown represents the granular quartz, supposed here to constitute the interior as well as the summit of the mountain. The dark colour represents the schistus; the two belts of grey are the limestone strata on the north and south sides. OR or the elevation of the south observatory above the lowest part of the base of the mountain is 1440 feet.

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* Phil. Trans. Vol. LXVIII. p. 751.

† The observatories O and P are not in the same meridian; they are however nearly so; and the section through P in the direction of the meridian would not differ sensibly from that which is here given

Fig. 3. is a section of the mountain in the direction perpendicular to the meridian of the south observatory. This section, though not referred to in any of the computations, is useful for enabling one to form an idea of the structure and figure of the mountain.

Draw OL (Fig. 2.) parallel to the horizon. With OR as an axis, and with a radius of 13333 feet, suppose a cylinder to be described, and let it be cut into two semi-cylinders by a plane passing through OR perpendicular to RQ the meridian. Then the whole of the mountain on the north side of this plane disturbs the direction of the plummet by drawing it toward the north. But the part of the mountain to the north of this plane, and between the levels O and R is equal to one of the semi-cylinders above mentioned, *minus* the empty space between the surface of the ground and the horizontal plane passing through O. If therefore S denote the attraction of the semi-cylinder, and V that which the void space would have were each pillar in it to consist of matter of the same density with the part of the same pillar which is under the surface, S—V will represent the attraction or disturbing force of all that part of the mountain which is north of OR and under the level of O.

Again, putting S' and V' to express the same things for the part of the mountain to the south of O, the whole attraction of that part equal $S' - V'$, and this acting in an opposite direction to the other, or tending to restore the plummet to its mean position, is to be subtracted from the former quantity, so that the whole disturbing force by which the part of the mountain below the level of O acts upon the plummet at O, is $S - V - S' + V'$. To this the attraction of the upper part of the mountain, or that which is above the level of O, being, as it happens, wholly to the north is to be added, and if it be called T, the whole disturbance on the plummet at O is $S - S' - V + V' + T$.

In Dr. HUTTON's computation, S and S', or the attraction of the half cylinders on opposite sides of O are equal to one another, the cylinder being supposed to consist of matter of the same density throughout; they must therefore destroy one another, and consequently, according to that hypothesis, they did not require to be calculated. The case here is not the same; for the matter in the two semi-cylinders not being of uniform density, nor having its inequalities similarly distributed, the attraction of each must be calculated in order that their difference, or $S - S'$ may be found.

If Σ, Σ', U, U', and T' denote the same quantities for he observatory P on the north side of the mountain, then the disturbing force on the plummet at P, $= \Sigma - \Sigma' - U + U' + T'$; and so the whole force which alters the direction of gravity is $S - S - V + V' + T + \Sigma - \Sigma' - U + U' + T'$.

The computation of these quantities for the columns in the quadrant north-west of O, will serve to explain the method followed in all the rest.

The whole cylinder of which OR is the axis being divided into 960 columns, the quarter of it must consist of 240, all of which, as far as their bases are concerned, are of equal force in attracting the plummet at O, so that the difference of their effects depends entirely on their altitude. Let O, (Fig. 1.) represent the south observatory, ON the meridian, and the quadrant ONW a horizontal section through O of one-fourth of the cylinder, on which the bases of the columns are marked as in the figure. Let *a b c* be the bounding line of the quartz projected on the plane of this section, the columns whose bases are within that line being supposed wholly of quartz, and those without it of micaceous schistus. If we suppose the columns that have their tops in this section to be extended downwards to the depth of 1440 feet, we shall have the quarter cylinder divided into 240 columns, that would be of equal disturbing forces, were they of equal density, and equal apparent depression below the point O. The inspection of the figure serves to distinguish the columns of quartz from those of micaceous schistus. In those columns which consist of both rocks, the proportion of the quartzy to the micaceous part could be judged of with sufficient accuracy by the eye. To assist the eye, however, the figure being first constructed to a large scale, I used to stretch a fine thread either in the direction of a radius passing through O, or in a line at right angles to that direction (according as the case seemed to require), so as to divide the quadrilateral into two quadrilaterals equal, as nearly as the eye could judge, to the irregular divisions made by the boundary of the quartz and schistus. The proportion of the parts was then easily ascertained. Now, by the first of the theorems laid down above, the attraction of any column on the plummet at O, estimated in the direction of the meridian ON, if b be the breadth of it in the direction of the radius, d the difference of the sines of the azimuths of the two edges, E the angle which the length of the column subtends at O, if its density were = 1, would be $bd \times \sin. E$. But if the density of the rock be expressed by any other number, the attraction just found must be multiplied by that number in order to give A the real attraction of the column. Thus if Q denote the density of the granular quartz, and M that of the micaceous schistus, we have in the former case $A = b\,d\,Q. \sin. E$, and in the latter, $A = b\,d\,M. \sin. E$. In these formulas $b = 666.66$ feet, and $d = \frac{1}{12}$, by the construction already explained; therefore $A = (55.55)\,Q \sin. E$, or $= (55.55)\,M. \sin. E$.

The calculation of sin. E is very easy, for the length of each column or its depth below O being 1440 feet, and the middle of the first ring being 333.33 feet distant from O; of the second 1000, reckoning from O, if n be the number of any ring, the distance of its centre from O is $\frac{2n-1}{3} \times 1000$, so that $\tan. E = \dfrac{1440}{\frac{2n-1}{3} \times 1000} = \dfrac{3 \times 1.44}{2n-1}$.

The sine corresponding to this tangent taken from the tables, and multiplied into 55.55, and the product into Q or M, will give the attraction of the column. Therefore to have the attraction of the ring of columns of the order n, the quantity now obtained must be multiplied by 12, that being the number of columns in one ring, having all by hypothesis the same altitude, so that the whole attraction of the ring = (666.66) Q sin. E, &c.

The attraction of each of the twenty rings being thus computed, their sum gives the attraction of the quarter cylinder.*

From the projection of the columns in Fig. 1. it appears that the first six rings in the NW. quadrant are entirely of quartz, that the five following are mixed, being partly quartzy partly micaceous, and that the nine remaining columns are wholly micaceous. The little table that follows contains the proportions of quartzy and micaceous rock in the five rings just mentioned.

	Sectors.	1	2	3	4	5	6	7	8	9	10	11	12
Rings.	7	$\frac{7}{10}q$ $\frac{3}{10}m$	$\frac{8}{10}q$ $\frac{2}{10}m$	$\frac{9}{10}q$ $\frac{1}{10}m$	q	q	q	q	q	q	q	q	q
	8	m	m	m	$\frac{1}{8}q$ $\frac{7}{8}m$	$\frac{2}{5}q$ $\frac{3}{5}m$	$\frac{8}{9}q$ $\frac{1}{9}m$	q	q	q	q	q	q
	9	m	m	m	m	m	m	$\frac{2}{5}q$ $\frac{3}{5}m$	q	q	q	q	$\frac{7}{11}q$ $\frac{4}{11}m$
	10	m	m	m	m	m	m	m	$\frac{1}{8}q$ $\frac{7}{8}m$	$\frac{7}{8}q$ $\frac{1}{8}m$	q	q	$\frac{1}{4}q$ $\frac{3}{4}m$
	11	m	m	m	m	m	m	m	m	m	$\frac{1}{4}q$ $\frac{3}{4}m$	$\frac{1}{4}q$ $\frac{3}{4}m$	m

This table is constructed only for that part of the north-west quadrant in which columns occur of two different rocks; and a rectangular cell is assigned to each column in the five rings to which the table refers. The letters Q and M denote quartz and mica; and where one letter only occurs, the column is entirely of the rock which it denotes. In the cells where both letters occur, the column consists of both rocks in the proportion expressed by the fraction prefixed to each letter. Thus in the seventh ring, the first quadrilateral is $\frac{7}{10}$ quartz and $\frac{3}{10}$ mica; the second, $\frac{8}{10}$ quartz and $\frac{2}{10}$ mica; the third, $\frac{9}{10}$ quartz and $\frac{1}{10}$ mica; the remaining nine being entirely quartz.

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* It was most convenient to compute the attraction of the quarter cylinder in this way, though merely an approximation, because the columns of which it consisted are not all of the same specific gravity. In the case of their being homogeneous, the attraction of the quarter cylinder might be computed *exactly* by the second theorems given above. Indeed I investigated that theorem for the purpose of examining into the degree of accuracy that this approximation actually possessed; and I had the satisfaction to find, that when the two methods were applied to the same half or quarter cylinder, (supposed homogeneous,) the difference of the results did not exceed a two thousandth part of the whole. This demonstrated in a very satisfactory manner the accuracy of the method pursued by Dr. HUTTON.

Now to apply the tables thus constructed to the computation of the attraction of any of the quarter cylinders, it must be observed, that sin. E is to be found for any column in the way already explained, and is then to be multiplied by *b d*, *b* being $= \frac{2000}{3}$ and $d = \frac{1}{12}$, so that $b\,d = \frac{2000}{3 \times 12} = \frac{500}{9}$, and therefore the coefficient of Q or M is $\frac{500}{9} \times$ sin. E.

When the whole ring is of the same rock, the coefficient of sin. E computed for a single column is to be multiplied by 12, so that the whole attraction of the ring $= \frac{500}{9} \times 12 = \frac{2000}{3} =$ 666.66, as before determined.

In the mixed columns the sine of E is to be multiplied both into *b d*, and into the fraction prefixed to Q for the quartz, and to M for the mica; or if we would include the whole ring, as E is the same for all the columns contained in it, we must multiply *b d* by the numbers denoting the proportion of quartz or of mica in the whole of that ring. Thus in ring 5, the first in the preceding table, the whole quartz = 11.4, the sine of E being $= .3157$, and the attraction due to the quartz $= \frac{11400 \times Q}{3 \times 6} \times .3157 = (129.9423)\,Q$.

In this manner the attraction of the whole cylinder on the plummet at O is readily computed; but it muct be diminished on account of the part by which this cylinder rises above the surface of the ground. The quantity that is to be subtracted is computed from the sines of the depressions of the tops of the different columns below the observatory O; and Dr. HUTTON's paper either actually exhibits* those sines, or furnishes us with the means of readily computing them.† When a ring is wholly of the same species of rock, the sum of the sines of the depressions of all the columns in that ring is given in the tables, and needs only to be multiplied by $\frac{500}{9}$ to give the coefficient of Q or M as far as that ring is concerned.

Again, when in the same ring some columns are of quartz and others of mica, the sines of depression must be computed trigonometrically for each column by help of the data contained in the tables above referred to. The sum of those sines for the quartz columns being multiplied by $b\,d$ gives the coefficient of Q.

Where the same column is of two different kinds of rock, the sine of the depression, or of E, must be multiplied into $b\,d$, and divided in the proportion of the numbers prefixed to Q and M in the cell belonging to the column.

All this may be illustrated by the calculation of the attraction of the columns belonging to the above table. In the seventh ring the first columns are mixed, the next three are entirely of quartz, and the remaining six are wanting, that is to say, their tops are not depressed below the level of O, as may be seen in Table V. of Dr. HUTTON'S paper. From that table it also appears, that the depth of the summit of the first column of the seventh ring below the level of O is 250 feet; of the second 240, of the third 200, of the fourth 150, of the fifth 60, and of the sixth 30. From these measures the angles of depression may be computed. Thus, if 250 be divided by the radius of this ring, viz. $\frac{13000}{3}$, we have .0577 for the tangent of the depression, or of E, and the sine which corresponds is .0568. As $\frac{7}{10}$ of this column consists of quartz, we must take $\frac{7}{10}$ of this sine for the proportional part of the coefficient of Q. In like manner, the sine of the depression of the top of the second column is .0545, of which taking $\frac{8}{10}$, we get .0436 for the part of the coefficient of Q belonging to this column. So also for the third ring, the proportional part of the sine is .04149. The fourth, fifth, and sixth columns being entirely of quartz, no proportional parts are to be taken; their sines, computed as before, are .0346, .0138, .0069; and the sum of all these six numbers is .18015.

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* Phil. Trans. Vol. LXVIII. from p. 769 to 776.

† Ibid. from p. 759 to 765.

Calculating in the same way for all the columns that are entirely or partly of quartz in the north-west quadrant, we have the amount of the whole = .2534. Now the total sum of the sines of the depressions in this quarter, is 13.534. (See Dr. HUTTON'S computations, page 83). From this number, if .2534 be taken away, there will remain 13.2806 as the coefficient of M, arising from the depressions of the micaceous columns.

Now the sum of the sines belonging to the quartz in the quarter cylinder itself has been found = 53.3532, from which taking away .2534, there remains .53.0998 for the entire sum of the sines belonging to the quartz under the level of O in the north-west quarter of the mountain.

In like manner the sum of the sines computed for the micaceous columns and parts of columns in the north-west quarter cylinder, is 23.0124, from which taking away 13.2806, the deficient or negative part, there remains 9.7318. The numbers thus found being multiplied by $\frac{500}{9}$, give the coefficients of Q and M for the north-west quarter of the mountain below the level of O, and make its attraction = (2949.99) Q + (540.655) M.

A similar computation being made for the quarter cylinder on the north-east of O, we have its attraction = (2974.299) Q + (577.98) M, to which adding their former attraction (2949.99) Q + (540.655) M, we have S—V, or the attraction of the mountain on the north side of O, and under the level of O = (5924.289) Q + (1118.635) M. In like manner the attraction of the south-west quadrant deduced partly from the quarter cylinder, and partly from Dr. HUTTON's calculations = (1049.18) Q + (1819.66) M, and of the south-east = (1567.394) Q + (1052.129) M; the sum of which, or S′—V′, gives (2616.574) Q + (2871.789) M; to be subtracted from the former, in order that we may have the total disturbing force of the part of the mountain below O, which is therefore = (3307.715) Q — (1753.154) M.

Lastly T, or the attraction of the part of the mountain above O (which is on the north), when computed from the sums of the sines of the elevation of the columns above O, as given by Dr. HUTTON, is found $= (2474.389)\,Q + (150.855)\,M$, which, added to the preceding, gives the whole attraction on the plummet at $O = (5782.104)\,Q - (1903.209)\,M$.

The same quantities calculated for P, the north observatory, are $(8061.022)\,Q - (3127.05)\,M$. To which adding the attraction just found for O, we have $(13843.126)\,Q - (5030.214)\,M =$ the total force of attraction increasing the convergency of the plumb line on opposite sides of the mountain.

Now if D be the mean density of the globe, it follows from Dr. HUTTON's calculations that $87522720 \times D$ is the measure of the attraction of the whole earth. But the Astronomer Royal having found by his observations, that the sum of the deviations of the plumb line on opposite sides of the mountain is 11.6 seconds, the attraction of the earth is therefore to the sum of the opposite attractions of Schehallien, as radius to the tangent of $11''.6$, that is as 1 to .000056239, or as 17781 to 1; or, making an allowance for the centrifugal force arising from the earth's rotation, as 17804 to 1. Therefore $17804 : 1 :: 87522720 \times D : (13843.126)\,Q - (5030.214)\,M$, so that $\frac{87522720}{17804}\,D = (13843.126)\,Q - (5030.214)\,M$, and hence $D = \frac{13843126 \times Q - 5030214 \times M}{4915902}$, or $D = (2.816)\,Q - (1.023)\,M$.

If we suppose $Q = 2.639876$ and $M = 2.81039$, as in the table above, $D = 4.55886$.

Dr. HUTTON makes $D = \frac{17804}{9933}$ multiplied into 2.5, the supposed density of the rock,* which gives $D = 4.481$, considerably less than the preceding. If in the formula $D = (2.816)\,Q - (1.023)\,M$ we make $Q = M = 2.5$, the result should agree with Dr. HUTTON's, and does so very nearly, making $D = 4.482$.

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* Phil. Trans. Vol. LXVIII. p. 781.

In all this, we have proceeded on the supposition that the granular quartz not only constitutes the summit of the mountain, or the part above the level of the observatories, but that it also descends into the interior of the mountain down to its base, where it is bounded by the curve line *a b e* (Fig. 1). On the other supposition mentioned above, that the granular quartz does not constitute the interior nucleus of the mountain, but is confined to the upper part of it, the rest consisting of micaceous schistus, our formula, after undergoing certain changes, may also be accommodated to this hypothesis. In the value of the attraction of the part of the mountain below O, viz. $(3307.715)\,Q - (1753.154)\,M$, we must suppose $Q = M$, when the above quantity becomes $(1554.561)\,M$. To this we are to add T, or the attraction of the part of the mountain above O, which remains the same as before, viz. $(2474.389)\,Q - (150.055)\,M$, to which if we add $(1554.561)\,M$, the sum $(2474.389)\,Q + (1404.506)\,M$ is the whole attraction on the plummet at O, according to this new hypothesis.

If the same changes are made with respect to the observatory P, we shall have the total attraction. Now the attraction of the part of the mountain below $P = (5593.347)\,Q - (3172.15)\,M$, which if $Q = M$ becomes $(2421.197)\,M$. Also the attraction of the part above P is $(2467.675)\,Q + (45.15)\,M$. If to this be added $(2421.197)\,M$ the amount, or $(2467.675)\,Q + (2466.347)\,M$ is the total attraction on the plummet at P.

To the total attraction at $O = (2474.389)\,Q + (1404.506)$

M, add the total attraction at $P = (2467.675)\,Q + (2466.347)\,M$; then the total attraction by which the direction of gravity is altered by the mountain, is $(4942.064)\,Q + (3870.853)\,M$. Hence as before $D = \frac{(4942.064)\,Q + (3870.853)\,M.}{4915.902.}$ or $D = (1.0053)\,Q + (0.78743)\,M$.

Here if we make as before $Q = 2.639876$ and $M = 2.81039$, we shall have $D = 4.866997$. This therefore is the mean density of the earth, on the supposition that the interior of Schehallien, on a lower level than the observatories, consists of micaceous schistus. The measure thus obtained, for the mean density or mean specific gravity of the earth, is above that of any of the precious stones, and is nearly a mean between the results of Dr. HUTTON and Mr. CAVENDISH. According to the former, $D = 4.481$; according to the latter, $D = 5.48$, the mean of which is 4.98. The difference between this and the last of our results is nearly $= .1$, or less than a forty-fifth part.

If we are to consider the experiments on Schehallien singly, it seems highly probable that the mean density of the earth is contained between the limits deduced from the two different suppositions concerning the structure of the mountain, so that it cannot be less than 4.5588, nor greater than 4.867. The mean of these is nearly 4.713.

It is however desirable that an element so important in physical astronomy, as the mean density of the earth, should be the result of many experiments. The principle on which those at Schehallien were made seems the most likely to lead to accurate conclusions. In the selection of the places fit for such observations, the homogeniety of the rock is a condition that merits particular attention, and is hardly to be looked for any where but among granite mountains, as they alone afford a perfect security that their interior and exterior are composed of the same materials. Granite is the lowest of the rocks, and whenever it appears at the surface we may be assured, that on penetrating deeper, we shall meet with no other.

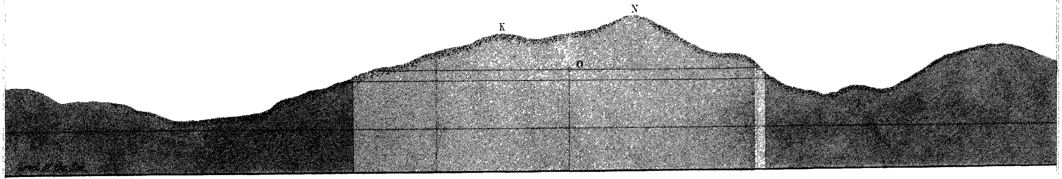
It is therefore to the primitive mountains, and among them to the granitic, that such experiments as those made at Schehallien ought to be confined. The want of homogeneity will then be on the outside of the mountain only, and can easily be estimated. The granite may be covered at the bottom of the mountain and even to a considerable height on its sides with beds of gneiss, mica slate, hornblende slate, &c., the quantity and position of which can easily be ascertained by observation.

XIX. *Observations and Experiments on Vision.* *By* William Charles Wells, *M. D. F. R. S.*

Read July 4th, 1811.

I. I WAS consulted, in the beginning of the year 1809, upon a disease of vision, which, as far as I know, has not hitherto been mentioned by any author. The subject of it was a gentleman about thirty-five years old, very tall, and inclining to be corpulent. About a month before I saw him, he had been attacked with a catarrh, and as this was leaving him, he was seized with a slight stupor, and a feeling of weight in his forehead. He began at the same time to see less distinctly than formerly with his right eye, and to lose the power of moving its upper lid. The pupil of the same eye was now also observed to be much dilated. In a few days, the left eye became similarly affected with the right, but in a less degree. Such was the account of the case, which I received from the patient himself, and from the surgeon who attended him. The former added, that previously to his present ailment his sight had always been so good, that he had never used glasses of any kind to improve it. On examining his eyes myself, I could not discover in them any other appearance of disease, than that their pupils, the right particularly, were much too large, and that their size was little affected by the quantity of light which passed through them. At first, I thought that their dilatation was occasioned by a defect of sensibility in the

Fig. 1.



Longitudinal section of Schehallien through the two Cairns N & K; *bearing from the Meridian N. 85 18½ W*

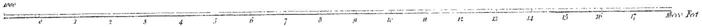
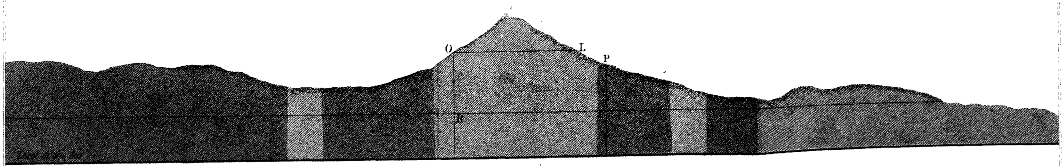


Fig. 2.



Section of Schehallien in the plane of the Meridian of O *the South Observatory.*

Engr. by J. Basire.

retinas; but I was quickly obliged to abandon this opinion, as the patient assured me, that his sensation of light was as strong, as it had ever been during any former period of his life. I next inquired, whether objects at different distances appeared to him equally distinct. He answered, that he saw distant objects accurately, and in proof told me what the hour was, by a remote public clock; but he added, that the letters of a book seemed to him so confused, that it was with difficulty he could make out the words which they composed. He was now desired to look at a page of a printed book through spectacles with convex glasses. He did so, and found that he could read it with ease. From these circumstances it was very plain, that this gentleman, at the same time that his pupils had become dilated, and his upper eye-lids paralytic, had acquired the sight of an old man, by losing suddenly the command of the muscles, by which the eye is enabled to see near objects distinctly; it being known to those, who are conversant with the facts relating to human vision, that the eye in its relaxed state is fitted for distant objects, and that the seeing of near objects accurately is dependant upon muscular exertion.

The disease of which I have spoken is perhaps not extremely rare. For having related the preceding instance of it to Mr. WARE, a Fellow of this Society, he was kind enough shortly after to send to me a young woman, who appeared to be likewise affected with it. But as I saw her only once, and had not then sufficient time to examine her case minutely, I speak with diffidence concerning its nature.

II. After I had reflected frequently upon these cases, it occurred to me, that, as the juice of the herb Belladonna, when applied to the eye, occasions the pupil to dilate considerably, and to become unalterable by light, an effect might at the same time be produced by it upon vision, similar to that which I have just described. I had, indeed, in the course of a few years immediately preceding, applied Belladonna several times to my own eyes, without observing any change in my sight, beyond what I referred to the increased size of the pupils; but as I had not looked for any other, I thought it possible, that some additional one might have happened, without my having perceived it. I resolved therefore to make the experiment anew. But to conduct it with precision, it was previously necessary to know, to what extent I possessed the faculty of adapting my eyes to different distances. On this subject I had made many experiments with great care, nearly twenty years before, and had ascertained,* that with my left eye, which was more perfect than the right, I could bring to single points on the retina pencils of rays, which flowed from every distance, greater than that of seven inches from the cornea. In the mean time, however, my eyes had altered considerably, with respect to their seeing near objects distinctly, and I had, in consequence, been obliged, not only to use convex glasses, but to change them several times for others of higher power. No dependance therefore being now to be placed in my former experiments, in regard to the present state of my sight, I repeated them, and found, to my great surprise, that the power I once possessed of adapting my eyes to different distances was entirely gone; in other words, that I was now obliged to regard all objects, whether near or remote, in the same refractive state of those organs. I found also, that my eyes, considered as mere optical instruments, were nearly the same as they had been in my youth, and that the convex glasses which I used did very little more, than supply, with respect to near objects, the place of a living power which I had lost, without compensating, except in a very small degree, for any alteration in the external shape of the eye, or any change in the configuration of its interior parts. I ascertained, for instance, that to give my left eye the refractive power which it formerly possessed while in its most relaxed state, that by which it was enabled to bring a pencil of parallel rays to a point on the retina, a glass of thirty-six inches focus was fully sufficient; whereas to produce an equal effect upon rays proceeding from a point at the distance of seven inches from my eye, the other extremity of my ancient range of perfect vision, I was now obliged to employ a glass having a focus of only six inches. I regret much, that I had not made such experiments frequently before, as I think it very probable, that I should have found a period in the progress of my vision to its present state, in which my capacity of seeing distant objects was the same as in my youth, and when therefore the whole of my imperfect vision of near objects would have been owing to a loss of the muscular powers of my eye.

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* Essay on Single Vision with two eyes, &c. p. 137.

As there can be no good reason for supposing, that the changes which have occurred in my eyes are different from those, which the eyes of by far the greater number of persons, who are not short-sighted, undergo at the approach of old age, it is evident, that the experiments of Dr. YOUNG* on the eye of HANSON, whom the learned author considered as a very fair subject for such trials, furnish no proof, that the want of the crystalline lens disables a person from having perfect vision at different distances; for as HANSON was sixty three years old, it is highly probable, that the results of the experiments would have been exactly the same, if he had still possessed that part of his eye.

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* Phil. Trans. 1801, p. 66.

III. Having discovered that my own eyes were unfit for the experiments, which I wished to be made with Belladonna, I instructed an ingenious young physician, Dr. CUTTING, from the island of Barbadoes, and now residing there, in the manner elsewhere described by me,* of ascertaining his range of perfect vision by means of luminous points. This he found, in consequence, to begin, with respect to his left eye, at the distance of six inches, and not to terminate at the distance of eight feet, beyond which he could not see clearly the object, with which he had hitherto made his experiments, the image of the flame of a candle in the bulb of a small thermometer. The flame of a lamp, distant about sixty yards, gave a faint indication of its rays meeting before they fell upon the retina; the rays from a star had very evidently their focus a little before that membrane. He now applied the juice of Belladonna to his left eye. Half an hour after, when his pupil was but little dilated, perfect vision commenced at the distance of seven inches; in fifteen minutes more, it began at the distance of three feet and a half. When his pupil had acquired its greatest enlargement, the rays from the image of the flame of a candle, in the bulb of a small thermometer at the distance of eight feet, could not be prevented from converging to a point behind the retina. The rays from lamps still more distant, and from stars, had their focuses at the same time on the retina. This state of vision continued, in its greatest extent, to the following day; and it was not till the ninth day after the application of the Belladonna, that he completely recovered the power of adapting his eye to near objects. While his left eye was thus affected, the vision of the right remained unaltered.

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* Essay on Single Vision, &c. p. 116.

Dr. CUTTING remarked, while his left eye was returning to its natural condition, that the diminution of the pupil, and the increase of the range of perfect vision, did not keep regular pace with each other; but that after his pupil had nearly returned to its former size, his capacity of adapting the eye to different distances was still very limited. As these effects therefore are not inseparably connected, they may occur in others in a different manner from that which he observed. A great degree of dilatation, for example, may take place in the pupil, without a total want of the power to adapt the eye to different distances.

Though I could not doubt the accuracy of Dr. CUTTING's observations, more especially as the altered state of his eye had lasted a considerable time, and as he had not been prevented by other occupations from attending minutely to the appearances, which were consequent upon it; yet, as he was the first person who had ever applied Belladonna to his eye for the purpose which has been mentioned, and as the results had been remarkable, I requested him to repeat the experiment with his other eye. He complied with my desire, and found, that the appearances which followed were similar to those, which had been produced by the application of Belladonna to his left eye.

It will, perhaps, be thought extraordinary, that Dr.

CUTTING's eye in its relaxed state, before the application of the Belladonna, brought parallel rays to a focus anterior to the retina; but that similar rays met in a point upon the retina, while the eye was under the full influence of that substance; as it may hence seem, that the Belladonna had done more than merely suspend the exercise of the power, by which the eye is fitted to see near objects distinctly. An observation drawn from the former state of my own sight will, I expect, make this matter plain.

When I enjoyed the faculty of adapting my eyes to objects at different distances, the rays of a star, which was viewed attentively by me, always met in a point a little before the retina;* whence I at first concluded, that my eye was unfit for accurate vision by parallel rays. But I afterwards found, that if I looked at a star carelessly, its rays had then their concourse on the retina. In the former case, from long habit, originating in my having chiefly viewed near objects with attention, some small exertion was made for the accurate view of a distant object, though none was requisite; in the latter, all demand for exertion ceasing, my eye fell into the most relaxed condition, that by which it was fitted for parallel rays. Dr. CUTTING's eye seems to have been similar to what my own once was, in regard to such rays; but as he had not acquired the faculty of viewing a distant object, without making some exertion, the rays from a star crossed one another in his eye before they came to the retina. The capacity, however, of making any exertion was taken away by the Belladonna, and pencils of parallel rays were, in consequence, brought to points upon that membrane.

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* Essay on Single Vision, &c. p. 138.

IV. Being now in possession of a new instrument, I next attempted to gain, by means of it, some illustration of the changes, which the vision of short-sighted persons undergoes from age.

It has been very generally, if not universally, asserted by systematic writers upon vision, that the short-sighted are rendered by age fitter for seeing distant objects than they were in their youth. But this opinion appears to me unfounded in fact, and to rest altogether upon a false analogy. If those who possess ordinary vision, when young, become, from the flatness of the cornea, or other changes in the mere structure of the eye, long-sighted as they approach to old age, it follows, that the short-sighted must, from similar changes, become better fitted to see distant objects. Such appears to have been their reasoning. But the course pursued by nature seems very different from that which they have assigned to her. For of four short-sighted persons of my acquaintance, the ages of whom are between fifty-four and sixty years, and into the state of whose vision I have inquired particularly, two have not observed that their vision has changed since they were young, and two have lately become, in respect to distant objects, more short-sighted than they were formerly. As the manner, in which this change has occurred, is unnoticed, I believe, by any preceding author, I shall here relate the more remarkable of the two cases.

A gentleman, who is a Fellow of this Society, became short-sighted in early life, and as his profession obliged him to attend very much to minute visible objects, he for many years wore spectacles with concave glasses almost constantly, by the aid of which he saw as distinctly, and at as great a variety of distances, as those who enjoy the most perfect vision. At the age of fifty, however, he began to observe, that distant objects, though viewed through his glasses, appeared indistinct, and he was hence led to fear, that his eyes were affected with some disease. But happening one day to take up, in an optician's shop, a single concave glass, and to hold it before one of his eyes, while his spectacles were on, he found to his great joy, that he had regained distinct vision of distant objects. With regard to such objects, therefore, he had lately become shorter-sighted than he had formerly been. But along with this change, another occurred of a directly opposite kind. For when he wished to examine a minute object attentively, such as he used to see accurately by means of his spectacles, he now found it necessary to lay them aside, and to employ his naked eye. He had become, therefore, in respect to near objects longer-sighted. The power, consequently, in this gentleman, to adapt the eye to different distances, is either totally lost or much diminished; but the point, or small space to which his perfect vision is now confined, instead of being the most remote to which he could formerly accommodate his eyes, as is commonly the case with the ordinarily sighted when they are becoming old, is now placed *between* the two extremes of his former range of accurate vision. The eyes of the other short-sighted person, a physician of considerable learning, whose vision has been altered by age, have been affected in a similar manner, but not in so great a degree.

As the only change, which had occurred from age in the sight of such of my acquaintance as were considerably myopic was a lessening, on both sides, of their range of perfect vision, I conceived that this might be the ordinary procedure of nature in such cases, and that it might be imitated, in a young short-sighted person, by the application of Belladonna to his eyes. I have hitherto not been able to obtain permission to make the experiment on any young person, who is very short-sighted. Two gentlemen, however, who are somewhat short-sighted, have readily submitted to it; one of them, Mr. BLUNDELL, a diligent and ingenious student of medicine; the other, Mr. PATRICK, a well educated young surgeon in London. The first experiment was on Mr. BLUNDELL, and the apparent result was, that the range of his accurate vision was considerably diminished at both ends, but not annihilated. Mr. BLUNDELL, however, afterwards informed me, that he repeated the experiment with more care in the country, and found, that in one eye the nearest point of perfect vision was moved forward about two-thirds of the whole range, and in the other about one-third; but that, with respect to both eyes, the most remote points of the ranges were unchanged. He added, that while one eye was under the influence of the Belladonna, the other became shorter-sighted than it had been before; but the difference was not so great, as to induce me to place entire confidence in the justness of his observation. I think it right to mention here, that from mistake I applied only two-thirds of the ordinary quantity of Belladonna to his eye, in the first experiment; and that he probably, in consequence of my example, applied no more when he made the second; as this might have been the reason, that during both experiments he retained, in part, the capacity of adapting his eyes to different distances.

The experiment on Mr. PATRICK was conducted by myself, after he had been frequently exercised in observing the extent of his perfect vision. The results were similar to those which had been remarked by Dr. CUTTING. The power of altering the adaptation of his eye, according to the distance of the objects viewed, was for some time entirely lost, and his sight became accurately fitted for such only, as were placed at the farther extremity of his former range of perfect vision. While one eye was under the influence of the Belladonna, the vision of the other was unaffected.

From these experiments it seems probable, that Belladonna will in no case produce the same effect upon a young short-sighted person, that age has produced in the two instances of which I have spoken. I expect, however, to have an opportunity of repeating the experiment on two persons, who are very considerably short-sighted, and I shall take the liberty of communicating the result to the Royal Society, together with some observations I have already made, and others which I hope to make, respecting those persons, who seem to retain to extreme old age the power of seeing perfectly, as far as the accommodating power of the eye is concerned, both distant and near objects; and of others, who, after being without this power for many years, appear to regain it at a similar period of life. Probably the making known my intention may facilitate its accomplishment, by inducing other Fellows of the Society to furnish me with opportunities of increasing my knowledge of these subjects. In the mean time, I shall offer a few words upon two other topics in vision, which seem to derive illustration from my experiments with Belladonna.

V. 1. Not only do the pupils move together, when both eyes are in a healthy state, but the pupil of one eye affected with gutta serena moves with the pupil of the other, as long as this remains sound. These facts are generally, but in my opinion erroneously, attributed to an immediate sympathy between the pupils. For when the pupil of one eye becomes dilated from the application of Belladonna, the pupil of the other, so far from dilating, becomes smaller. It follows, therefore, that the size of the pupil is dependant, not only on the impression of light on the retina of its own eye, but on that also which is made on the retina of the other, and that the moving of the two together, which for the most part takes place, is only an accidental consequence of the fact which I have mentioned.

2. As the action of the external muscles of the eye has been frequently resorted to, for an explanation of its capacity to see objects perfectly at different distances, I requested Dr. CUTTING to attend to this matter. He accordingly ascertained, while his eye was in its natural state, the distance from his face of the nearest point, at which he could make the two optic axes meet, this being the greatest trial of strength, to which those muscles can be exposed. Shortly after, he repeated the experiments, while, in consequence of the application of Belladonna, he was without the power of adapting his eye to different distances, and found, that the strength of those muscles was not diminished. It follows, therefore, not only that the external muscles have little or no concern in fitting the eye to see distinctly at different distances, but that the same is true with respect to the cornea, as we cannot suppose, that its mechanical properties were altered by the Belladonna, or at least, that it became more inflexible from the application to it of the juice of that herb. I had before made a similar experiment on myself, by comparing what had been the strength of the external muscles of my eyes twenty years

ago,* with what it was after I had lost the power of altering their refractive state; but though I found no difference, yet, as their coats might have in the mean time become more rigid, I thought it right to have the experiment repeated, in a manner to which no objection could be taken.

The only other part of the eye, or its appendages, which remains for enabling us to see equally well at very different distances, is the crystalline; and that it does produce this effect, either wholly, or very nearly so, is manifest, from the necessity even young persons are under, who have lost it, of using glasses of very different convexities for near and remote objects. But in what way this important office is performed by it seems still unknown. The learned Dr. YOUNG, indeed, as well as others before him, has supposed, that the crystalline has the power of altering its figure; but the proofs hitherto given in favour of this opinion appear very defective. In 1794, I attempted to submit its justness to the test of direct experiments, by applying to the crystallines of oxen, which had been felled from thirty seconds to a minute before, chemical and mechanical stimuli, and those of Galvanism and electricity; but in no instance was any alteration of figure, or other indication of muscular power, observed. All of these stimuli were applied to the crystalline while it was surrounded by air, and some of them while it was covered with warm water. Last summer, after I knew that men lose, from increase of years, the faculty of altering the refractive state of the eye, I thought it possible, that the oxen on which I had made the experiments were too old for them. I therefore repeated most of them on the crystallines of a calf and a lamb; but still no motion was to be seen. Dr. Young has made similar experiments with a similar event; but he thinks that no argument can hence be derived against his opinion, as neither can motion be excited in the uvea, by any artificial stimulus. In the first place, however, it is not agreeable to just reasoning to regard an unknown thing as an exception to a general rule, rather than as an example of it; in the second, the motions of the uvea are involuntary, whereas the adaptation of the eye is, in part at least, under the command of the will; and in the third, the crystalline seems very unfit for performing the motions which he assigns to it; for if its figure be altered out of the body, by external force, it does not restore itself, but retains the shape which has been given to it, like a piece of dough, or soft clay. Possibly further experiments with Belladonna may contribute to remove the obscurity, which at present surrounds this subject.

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* Essay on Single Vision, &c. p. 136.

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